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THE SOONKÊSALA CANAL.

DISCUSSION OF MR. JOHN HERBERT LATHAM'S PAPER.

Proceedings of the Institution of Civil Engineers.

II.

General Strachey, R.E., thought the paper was exceedingly valuable, and explained extremely well the great work on which the author had been engaged in Southern India. His experience had been confined almost entirely to Northern India, and there was very little he could usefully say on the engineering questions which were at issue. If, in the course of the discussion, reference was made to matters not of a specially engineering nature, but touching the general subject of irrigation in India, he would make such remarks as occurred to him.

Sir A. Cotton said he had had fifty years of Indian experience, and could speak with some authority on this subject. The Soonkesala canal was a grand work on these grounds: First, it was a large work in the upper country. Irrigation works had been executed by the Government in the deltas of the rivers and in the interior plains, but they were very different in their nature from this, which was carried through a vast, rocky, undulating country with a great fall. In the next place, it was the first instance in India of a thoroughly well-projected and well-constructed system of steamboat canal 190 miles in length. Other navigable canals of considerable extent had been constructed, and the canals of the Godavery could be navigated by steam-vessels, but these were not so complete as they ought to be. Even in America there was no steamboat canal to this day. With respect to the projection and execution of this work, he had examined all the papers that had passed between the Government and the company. It was a matter of notoriety that there was a strong prejudice on the part of officials in India against enterprizes of this kind, and the papers he had seen all bore the stamp of that feeling. It was therefore quite safe to consider that in those papers any mistake in projection or execution would be brought forward, and the tone of the papers could not be mistaken. Going through them, he never was more satisfied that there was no considerable mistake either in the projection or in the execution of these works, and that they were thoroughly effective. Numerous errors were pointed out, but they were mere trifles, not affecting in the slightest degree the real, fundamental points of the work. It was a great satisfaction to him to find that it was so. The feeling

was, and had always been, exceedingly strong against private enterprise in India, and every effort had been made to suppress the present company. As a rule such works got entirely into the hands of the Government; but he considered it was extremely important that these works should be entrusted to private companies, for, if any questions arose, there was no appeal in the case of Government works. If the people thought themselves aggrieved in respect of the arrangements about water or the price, they appealed from the power which they considered treated them injuriously to that same power. They had no redress. This had been forced upon his attention repeatedly in India, because several times when he had tried to defend native rights, he had found it impossible to do so, for the authorities were the very persons they had to complain against; and he had been baffled again and again in that way. Now if a company sold the water, and the native considered himself aggrieved, of course he could appeal to the Government, and, what was more, the bias of the Government was always on the side of the native. The natives themselves were extremely sensible of this. He could not conceive what the feeling was against these works being in the hands of a company. This work now consisted of a canal 190 miles long, leading the water of the Toongabudra into the valley of the Pennêr, and by that river to the district of Nellore, where there was a vast extent of native works which had been improved by the Government, and which would receive any water that was not used on the line of the canal. The project was complete in itself in a certain sense. It would irrigate 250,000 acres of rice during the monsoon, and 750,000 acres of what was called dry-grains after the monsoon. Moreover, there were 190 miles of navigation to bring the produce down to a certain point. But two things were required in addition; first, the navigation should be completed from the end of the canal to the beginning of the Government canal at Nellore, a length of eighty miles. When that was done there would be water carriage from the head of the canal to Madras, about 400 miles. Without this the work would be in a certain sense imperfect; that was to say, at the end of the canal the produce would have to be delivered over to the railway, to be carried, at an expense it would not bear, to Madras. If, however, the water transit were completed, it would open the whole of this highly-irrigated country, not only to Madras with its 500,000 inhabitants, but also to all the markets of the world. Therefore he considered the first thing necessary to be done to complete the project was that line of water communication. The other requirement was to store water to keep the canal supplied for the five months during which the natural supply failed. Water could be stored economically all over the valleys of the Toonga and Budra, in quantities sufficient for an additional million of acres during the dry season. Those two works could be executed at prices which would greatly increase the general return of the whole project, though the works as they now stood were, in his opinion, abundantly secure of getting a large percentage upon the outlay.

A minor point he would allude to was the cost of making these canals navigable. In this case there was a gradient of 450 feet in 190 miles, or 2½ feet per mile, and the outlay had been £150 per foot of fall; therefore with 2½ feet fall per mile it was £375 per mile. In considering the expense of transit works in India he would refer to the grand standard, viz., the cost of the railways in that empire. This had amounted on the average to £20,000 a mile, while the cost for lockage on this interior canal was, as he had stated, about £375 per mile—an insignificant item in comparison.

There was now open in India an unbounded field of enterprise and employment for the Civil Engineers of England, and the Madras Irrigation and Canal Company had done sensible benefit to the country in thus opening their works, and he hoped they would be permitted to complete the whole project which he had sketched out.

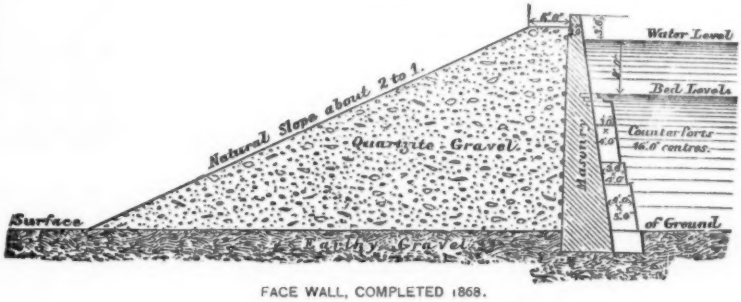
Even in England, where the distances were comparatively insignificant, the cost of transit was an important question; but in India it was of vital importance, where materials or produce had to be conveyed 500 or 1,000 miles to reach markets and ports. As yet almost the whole of India was shut up from the

want of cheap water carriage. The railways could not properly open up India. They were carrying goods at 2*d.* per ton per mile on an average, and this only by being backed up by a heavy unnual subsidy from the Government, and that charge of 2*d.* a mile was, in fact, a complete failure. India could not be developed under such a system; it must have cheap transit. He wished generally to express his opinion on this subject as an officer of long experience and a Government official. In many respects he stood in a peculiar position with regard to these works. He neither projected them, nor had he anything to do with their execution; nor was he in any way connected with the Madras Irrigation and Canal Company. He spoke as an unbiassed individual; but he could not refrain from expressing his satisfaction at seeing this extensive work completed and brought into active operation.

Irrigation works in India were now projected, and all but two or three of them were in process of execution, which would be completed at a cost of thirty-five millions sterling in irrigation and navigation, so that there was really an extensive work going on in this essential point.

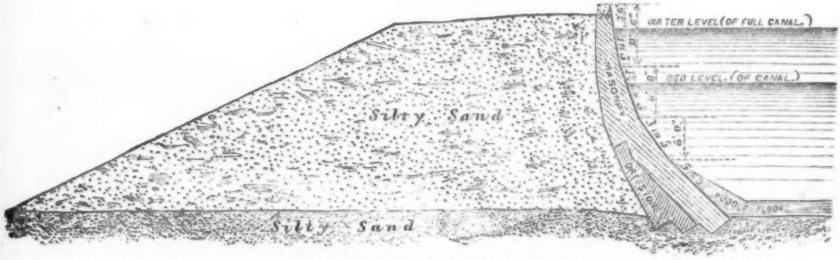
Mr. Bateman said there could be no question as to the great importance of this work, both as a work itself, and with respect to the advantages it must confer upon India; but he confessed to being somewhat surprised at the sections of the embankments. He was himself rather an old hand at making reservoir embankments; but he must say that he dare not construct reservoir banks in the way these canal banks appear to have been constructed. Fig 8 represented an embankment thirty feet in height with a wall two feet thick at the top, and not more than six feet thick at the bottom.

FIG. 8.



FACE WALL, COMPLETED 1868.

FIG. 9.



MASONRY REVETMENT, COMPLETED 1865.

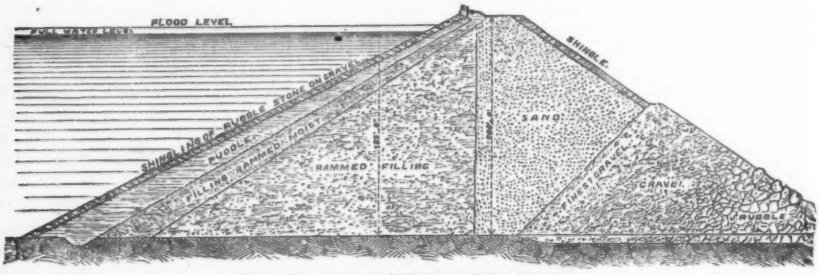
He could not comprehend how that bank, even with the water in the canal, could stand at all. If it did, it was a new plan of engineering, and was something from which engineers might learn, and possibly copy, for the future. He was quite sure no railway engineer, much less an hydraulic engineer, would venture to

build a wall thirty feet high, two feet or two feet six inches thick at the top, and six feet or so at the bottom, and expect it to stand.

Then, as to the masonry revetment (Fig. 9), founded on sand, with no protection or provision to prevent the water from the canal passing through into the bank; that wall, judging from the scale, could not be more than about two feet six inches thick, and again he could not understand how that stood. If it did, it was a valuable piece of experience in engineering. He would like to ask the author what was the character of the sand on which that wall stood. If it were pure sand, a depth of water upon it of thirty feet would cause it to act simply as a filter, and the water would escape. If it were argillaceous matter, mixed with sand, it might be so made as to be impermeable to water, particularly if it were made by basket labor and padded over, as on the Continent. If so made then he could suppose an embankment might be formed without puddle but impervious to water. Still the material of a bank had a particular slope at which it would stand without thrust; all above that slope would exercise a pushing force or pressure from behind, and such a revetment wall supported as shown he should scarcely expect to stand.

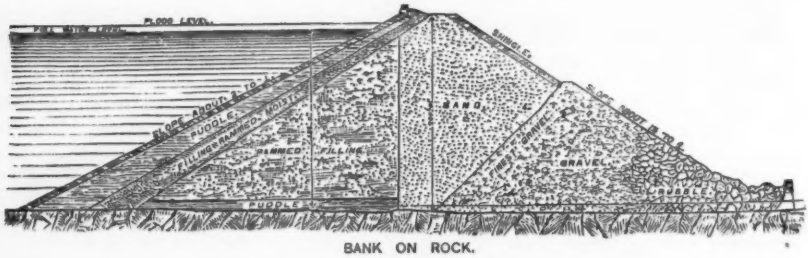
Again, three sections (Figs. 15, 16, and 17) were recommended for adoption, as

FIG. 15.



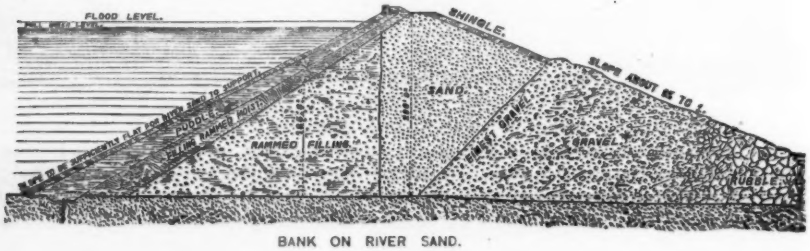
ORDINARY BANK ON STRONG IMPERVIOUS SOIL.

FIG. 16.



BANK ON ROCK.

FIG. 17.



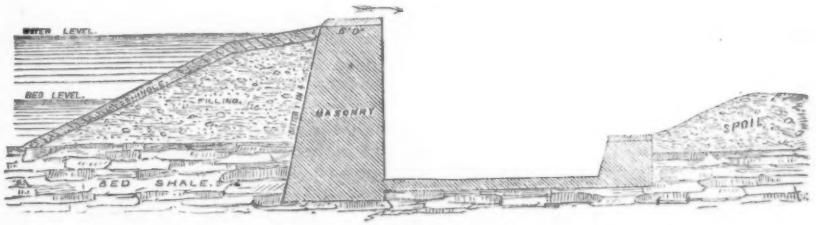
BANK ON RIVER SAND.

the mode in which every embankment should be formed, by puddle walling on the face or slope of the embankment, without this face puddle being tied into any material at the bottom or foot of the slope. The great difficulty in constructing water-tight banks in this country was the character of the ground on which the embankment had to be formed. With the right material, and with the materials properly put together, a water-tight embankment might readily be made; but there was great difficulty in preventing the escape of water below the embankment, through the natural ground on which it rested. If it were, though sandy, a water-tight and somewhat argillaceous mass of alluvial deposit, such as he could imagine would be found over a great portion of India, then there was not only a good material, but a material which could not, except by unskilful management, make a bad bank. If it were put together in thin layers, well trodden, it must inevitably make a water-tight mass, which would be safe. In this country where there were not such good materials, there was danger in putting puddle on the face of an embankment. The embankment, except it were made of sand, gravel, or stone, would gradually settle, the facing of puddle would crack, and the water would escape. These were his experiences; and he should be afraid to trust to less precaution than practice had taught him to adopt.

By way of illustrating how an embankment might be made perfectly safe, the author had suggested an embankment 200 feet in height, through which the water should leak, but so as not to destroy it. Engineers usually endeavored to construct banks water-tight in themselves, without any leakage. It was a new doctrine that embankments to be safe should be leaky, though he had known several cases of reservoirs so leaky that they were never filled, and consequently never washed away. But it did not answer to make leaky embankments. The ordinary filtering medium in this country was about four feet in thickness. The quantity which he understood the author to say would filter through this embankment was 500 gallons per square yard: that would be little short of sixty gallons per square foot. He did not know in what time that would take place; but a good filter would allow seventy-five gallons per square foot to pass in twenty-four hours. The water to be filtered rested on fine sand, which was supported by a coarser medium of sand, and that again was supported by a coarser still, till it got to round boulders as large as turnips and potatoes. That filtration was carried on under only one foot or two feet of water pressure; or, in the case of the London Water Works, three feet or four feet. Where the water was 200 feet deep, he should be sorry to contemplate the result in a work for which he was responsible. In making these remarks he did so only to elicit further information, as to the character of the material on which these banks were constructed, and of which they were made.

With regard to the masonry of the weirs, one of them appeared to have been placed upon red shale (Fig. 5).

FIG. 5.



JUTON WEIR, COMPLETED 1869.

Now shale was not a durable material with water falling vertically upon it; and unless the masonry floor was very firmly fixed, the drop of the water from a height of fourteen feet six inches would suck out that masonry sooner than

it would in that weir which was of greater height, but was placed upon the much harder gneiss rock (Fig. 3). The

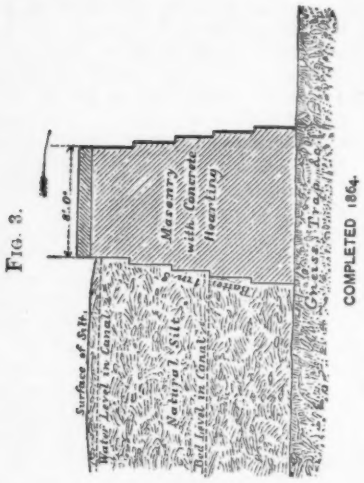


FIG. 3.

safety of such a work depended entirely upon the character of the rock over which the water fell. As a rule, his experience was that a perpendicular face was not a judicious form for constructing a weir, and that unless the work was built on substantial rock, the foundation was liable to be injured. A sloping weir, which allowed the water to fall gradually to the foot, was a safer mode of construction. He should be glad to hear how the sand and gravel in these banks were put together. In some valleys in Spain in which he had constructed weirs, the material consisted of sand or gravel of such an argillaceous description that it was hardly possible to make a bad bank. But if the embankment was to be formed on ground not itself water-tight, and if it had to be raised forty or sixty feet high, it required a large amount of skill and caution to secure a good foundation.

Mr. Robert C. Reid contended that an embankment wholly of earthwork and puddled clay would make a safer and more lasting structure than a composite one of masonry and earthwork. The method proposed by the author for constructing embankments which could leak without detriment to their stability might be theoretically correct, as far as the bank above the natural surface was concerned, but it was difficult to see how such a structure could be tied into the ground on which it stood so as to be safe. The site for such a bank must be prepared, either by excavating the whole material under the base down to an impervious stratum, and rearranging it according to the plan adopted for the portion above the surface, or a puddle trench would have to be dug and filled with clay all round the inside toe of the bank, where the finest material was placed. In order that the embankment might act as a filter-bed, the different materials must shade into each other, from fine sand to rubble, by minute gradations, or else the sand would be blown by the water through the rubble; and the process of selecting the available materials, by riddling or otherwise, and placing them in their proper positions, would be very costly. Provided the gradations were perfect, the finer the particles at the inside, the less would be the leakage, and if they were very fine, as in clay, there would be no leakage at all. There would therefore be great varieties in the retentiveness of different embankments built in this manner, and indeed in different portions of the same embankment.

It would be almost impossible to predetermine how much a given embankment would leak, and there would be no certainty of having sufficient command over the water in the reservoirs, so as to regulate the supply from it.

As regarded the construction of water-tight embankments, the plan of placing the puddle wall along the inside slope was not so good, in Mr. Reid's opinion, as the vertical wall in the center of the bank. More puddle would be required, since the area of the slope was greater than the vertical area, while the puddle trench having to be carried round the toe was longer than if it were in the center line, and, in the event of settlement, the puddle if at the face, being inclined to the direction of subsidence, was more liable to crack than if it were in the line of subsidence, as it would be if vertical.

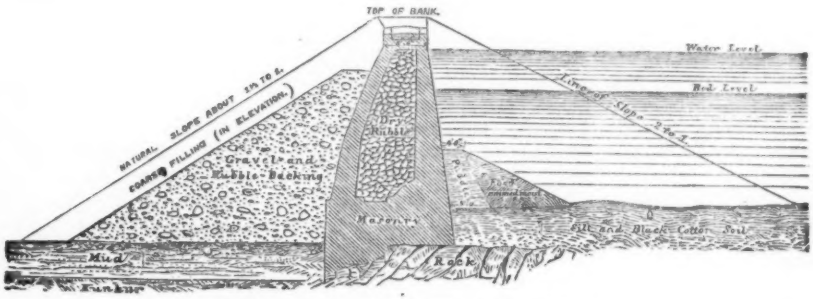
The proper function of the puddle wall in an embankment was to stop the 'creep' of water, which was inevitable in all earthwork unless wholly constructed of clay or other impervious material. Whenever water acted upon

clay in any volume, with the slightest motion it disentegrated the particles. Now this was the condition which the puddle would be in, if at the inner surface, with nothing but rubble and gravel between it and the water; for every wave in the reservoir would have its corresponding undulation in the water lying against the puddle wall, and would eventually in this manner eat into the clay. But, by placing the puddle wall in the center, there was a great thickness of earth between it and the water, and the minute tubes which carried the water through the earthwork effectually retarded the action of waves in the center, as well as prevented the direct pressure of the water on the puddle. If, however, the earthwork of the inner slope was of open, stony material, tipped in layers of five feet or six feet thick, and the stones were allowed to roll down to the bottom of each tip, it would not have the desired effect in protecting the puddle wall. Hence it was, that experienced engineers specified banks to be built in thin layers, with the most clayey portion of the material inside and next the puddle wall. On the outside slope the material next the wall should also be clayey, so that it might bear with the same effect as the inner slope on the wall, and keep it in equilibrium. The layers should also be sloped inwards to the center, so that the beating and consolidating would tend to press the earth against the puddle wall with as great force as the water of the reservoir would when full. If the outside slope was not capable of exerting such a pressure through an infinitely small space, it could not sustain that pressure; and whenever the water rose, it would yield, the puddle wall would bend, and, having no elasticity, would crack and cause the destruction of the bank.

There was hardly any limit to the height an embankment might be raised, as far as its power of resisting the pressure of the water was concerned; but the site on which it was built might not be able to carry the extra weight, and might bulge out above and below the bank. The slopes, which were sufficient for ordinary heights, would hardly be safe when the mass of artificial earth was increased to such an extent as was proposed.

Mr. A. J. Dodson, who had been engaged for eight years on this canal, said that, except the addition of a puddled foreshore, and the alteration of the outline of the bank in the rear of the masonry, the wall (Fig. 7), and the

FIG. 7.

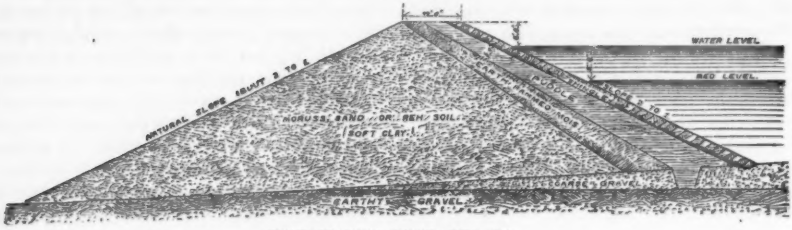


CANAL WALL, COMPLETED, EXCEPT THE PUDDLE, 1865.
PUDDLE AND WING ADDED 1871.

masonry revetment (Fig. 9), and all others of which they were types, were completed in the spring of 1865, and the puddle-core walls in the autumn of the same year. The length of canal in which the loftiest specimens of each kind occurred was opened for trial in 1866, without accident. The face walls (Fig. 8), 10,518 feet in total length, and the puddled banks (Fig. 10), were completed by Midsummer, 1868; water was passed down in July, and the canal navigated for sixty-five miles in August of that year, without doing them the slightest injury. A portion of ordinary bank, thrown up without selection, did, however, give way in that month.

Again, the walls represented in Fig.

FIG. 10.

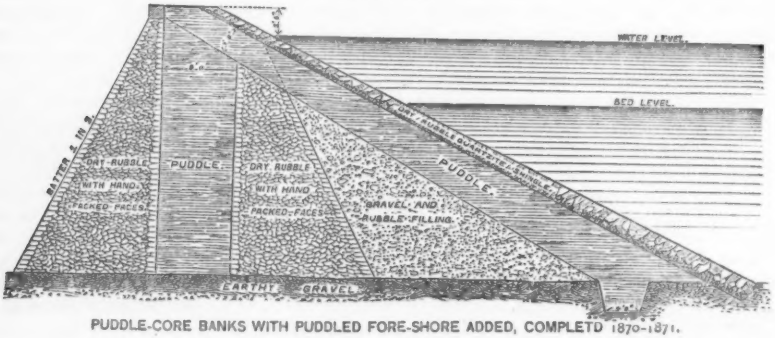


PUDDLED BANK, COMPLETED 1868.

7 were completed in 1865; no failure or accident occurred to these except during the extraordinary storm of August, 1870. One wall on the twenty-fourth mile was then overtopped, and, as there was no protection to the bank overlying the ends of the wall, a breach occurred at the junction, carrying away about 100 feet of the bank, and something less of the wall. Another wall, in the twenty-eighth mile, of narrower section than that shown in Fig. 7, also gave way, for the length of a chain or two; and consequently all the length of the narrow section of wall—a few chains only, and no great height—had been since specially protected in the front and rear. Fig. 9 represented a masonry revetment completed in 1865, above one mile in length. No failure had occured to this revetment beyond cracks, except during the storm of 1870, when it was overtopped by the water, and the bank washed away from behind, so that a breach followed 220 feet long in one place. The scour of the water towards the breach set against the toe of the revetment in another part, where a length of about four chains slipped into the canal, but no breach occurred there.

Puddle-core banks, (Fig. 11), were liable to leak. One in the thirtieth mile,

FIG. 11.



PUDDLE-CORE BANKS WITH PUDDLED FORE-SHORE ADDED, COMPLETD 1870-1871.

only ten feet high, on fissured rock and gravel, had the lower part of its core washed out on the 29th of July, 1868. Puddle foreshore was then added, and no failure had since taken place. They were the least satisfactory, perhaps, of any of the sections, though they had never been breached. The banks shown in Figs. 8 and 10 had given no trouble, and they survived the storm of August, 1870.

The above mishaps were spread over a period of six years, and a distance of sixty-five miles. The first section was exposed to the action of river and land floods; the second, third, and fourth, to large waves on tanks, especially at Tongaduncha tank; in the fifth section land floods spread five miles against the upper banks ten or fifteen feet deep. It was to be remarked that the puddle was not of the best nature. It consisted of

black cotton soil, which contained little clay. He thought the wonder was that with these difficulties there had not been even more accidents.

Mr. Latham had stated that two cubic yards per hour per acre was the datum on which his calculation of the amount of water for irrigation had been made. Colonel Cotton insisted on one cubic yard on the Cauvery and Colleroon irrigation works which he constructed; and Sir Arthur Cotton said that in the Tanjore and Godavery works 1½rd cubic yard was sufficient, or 4,000 yards for the 120 days of wet cultivation, and 1,500 cubic yards for dry crops. If that were the case the capabilities of this canal were far greater than those named in the paper.

With regard to the navigation of the canal, the author had stated what he imagined would be the amount of goods conveyed. That, however, would be only limited by the carrying power of the railway from Cuddapah to Madras, into which this canal now worked: but he was sure there would soon be a large amount of traffic in passengers. He had been up and down the Godavery canal, and he was astonished to see the amount of native passenger traffic from one place to another, in districts much less inhabited than those served by this canal. He confidently believed that both in an engineering and in a commercial point of view the Soonkesala canal would prove to be a success.

Mr. G. H. Phipps observed that the particular point which interested him was, as to whether canals in India could be made to contribute largely to the means of intercommunication in that country. From the observations of Sir Arthur Cotton he appeared to be under the impression that railways, which he had assumed had cost £20,000 a mile, could not do much more for intercommunication in India than irrigation canals. Mr Phipps thought it might be well worth while, where such a canal as the present one, costing £8,000 or £9,000 per mile, had to be constructed, to render such a canal navigable, if it could be done, as stated in the paper, at such a moderate additional outlay as £375 per mile. He could not, however, conceive the idea of canals superseding railways as a general mode of communication over large tracts of a country like India. Although not in all cases, yet in a great many, particularly in the transport of troops and military material, speed of transit was a very necessary element, and in this instance, not only would the canal be affected by the ordinary causes of slow traveling, but this evil would be increased by the current against which the return boats would always have to contend. Assuming, however, that railways, and not canals, were to afford the means of traveling in India, the great thing was to construct them cheaper, so as to cover more ground, and thus tend to increase the trade and prosperity of the country. Their cheapness would possess the further advantage of comparatively easy renewal after being swept away by the heavy floods so fatal to such works in India. He submitted that what was wanted for India was an extensive system of railways constructed upon a cheap plan, for he never could believe that navigable irrigation canals could form an adequate means of intercommunication in that country.

Mr. Bruce said, on some of the drawings banks fifty feet high were represented as having been built on sloping ground; while on the high side he understood there was hardly any bank at all. He did not see why it was necessary to make walls of that height, if by slightly moving the direction of the canal laterally this could have been avoided. He thought Sir Arthur Cotton had injured his advocacy of canals by contrasting them with railways, because in India both railways and canals were wanted. It had been asserted that it was proposed to make an extension of this canal at the rate of £375 per mile, that the Indian railways had cost on the average £20,000 a mile, and that the latter could only carry goods at 2*d.* per ton per mile, while the canals could carry goods a great deal cheaper; and that therefore, in the proportion of £20,000 to £375, railways were worse than canals, independently of the greater cost of carriage by the former: but the fact was this very canal had cost about £9,000 a mile.

Mr. Phipps remarked that Mr. Bruce was under a mistake. It had not been stated that the canal could be made for

£375 per mile, but that that amount would render an irrigation canal navigable.

Mr. Bruce said, he understood that the extension to Nellore was to cost £375 per mile for navigation, while this canal cost nearly £9,000 per mile; but if the £375 was only to do a portion of the work of making the canal, so much the greater the unfairness of putting that in contrast with the entire cost of the railways. In the Presidency of Madras the railways had not cost £20,000 a mile, and this estimate of £375 per mile had been put against the cost of carrying the railway across the Ghâts, instead of taking the cost of the railways in the presidency, which he believed did not exceed £12,000 per mile in the case of the Madras railway, and in the Great Southern it was the same as this canal. Of course rolling stock was included in the case of the railways in that amount, not so in the canals.

Lieut. Col. J. G. Medley, R. E., remarked on some points of detail in which this canal differed from those in the N. W. Provinces of India, that such comparisons were useful if only because the Engineers of Northern and Southern India had so little opportunity of seeing one another's works.

He should be glad to know if there were any masonry falls in the main canal, and if so, what form of fall had been fixed on? In Northern India, the vertical fall with a grating was now admitted to be the best yet devised. A cistern of water was sunk below the level of the lower bed, on to which the water fell as on an elastic cushion; while its force was also broken by its being first made to pass through a set of bars fixed obliquely in the crest of the fall, similar to the teeth of a comb. If there were no falls, the profile of the country must be very favorable, and the difficulties of navigation would be considerably lessened; but the combination of irrigation and navigation in the same canal involved difficulties which had never been successfully overcome in Upper India.

He inquired whether the under-sluices in the weirs placed across the main channel at the heads of the branch canals were found sufficient to prevent silting up? In the N. W. P. of India, regulating heads with wide sluices would be required across both channels at the points of bifurcation. He also asked whether the irrigation was all surface irrigation, or whether the water had to be lifted? And whether the water was delivered into the village water-courses from the main canal, or by intermediate distributaries? It was stated that measuring sluices were not needed. In the N. W. P. the difficulty had always been how to insure a constant discharge with a head of pressure continually varying; and though various attempts had been made to overcome this difficulty, they had been unsuccessful, owing chiefly to the flatness of the country preventing there being any spare fall beyond what was actually required for the flow of the water. Hence the water rate was always assessed on the area irrigated, instead of on the amount of water delivered—an objectionable method, as it led to waste of water, and to continual expense in measuring irrigated areas, besides opening the door to disputes and interference with the cultivator's arrangements.

With regard to the formation of the banks, it might be noted that the great embankments by which the Ganges canal was carried for three miles across the Solani valley, at an elevation of from ten to forty feet above the bed, were formed of earth, with a double revetment of brick masonry built in steps. These had stood well, and there was no leakage. Puddled banks, without further protection, would be perforated by rats in Northern India, and there was no stone or gravel to protect the puddle by pitching: brick was the only material available.

It was estimated by Mr. Latham that two cubic yards of water per hour were only sufficient to irrigate one acre of rice. This was only one-fourth of the irrigating duty now obtained from canal water under favorable circumstances in the N. W. P. when applied to wheat, the great staple of Upper India. One cubic yard per hour would there irrigate two acres; or, to use N. W. P. nomenclature, one cubic foot per second would be thought fully equal to the irrigation of 267 acres of wheat, in any canal where the irrigation was fully developed. On the Eastern Jumna canal, the most

perfect in this respect in Upper India, 306 acres were irrigated in 1868-9. On the Ganges canal, where the distributaries were not yet completed, only 232 acres.

Of course the amount of water required for rice was very different from what was wanted for wheat; and although there was some rice irrigation in Upper India, it was in the autumn, when there was plenty of water available; whereas the chief crop, or wheat, was watered in the cold weather, when water was most scarce and precious. The water-rate assumed in the paper, Rs. six per acre, would seem very high in Northern India, where sugar was the only crop that paid as much as Rs. five per acre for surface irrigation (Rs. 3⅓ only if the water had to be lifted). Rice paid Rs. three only, and wheat Rs. 2.4. For so large a discharge at the head of the canal as 400,000 cubic yards per hour, or very nearly 3,000 cubic feet per second, the length 190 miles, seemed very small. This was the calculated discharge of the Baree Doab canal, which when completed would be 470 miles long; and this difference made it of little use to compare the cost of canals under such different circumstances as those of Northern and Southern India. He had roughly estimated the cost of an irrigation canal in Northern India at about Rs. 4,000 per cubic foot per second of calculated discharge, or Rs. 30,000 per mile of length. The Soonkesala canal had apparently cost Rs. 50,000 per mile, or Rs. 3,330 per cubic foot per second.

Mr. Bateman said, the account of the failures which had taken place afforded some confirmation of the fears he entertained from looking at the sections of the embankments. With reference to the quantity of water required for irrigation, a cubic yard of water per acre per hour was equal to a depth of one-sixth of an inch of water per day. In the south of Europe, in Valencia and Murcia in Spain for instance, where there were still many of the old Moorish irrigation canals at work, the ordinary irrigation amounted to 2½ inches in depth, renewed every fortnight in certain periods of the year, which was about the same quantity as was used in this case, if put on the land once a fortnight instead of daily; but rice required four or five times as much water as the ordinary crops of the country, and for about ten weeks of the year.

Mr. Redman observed that the remarks upon the formation of the embankments, rather confirmed the propriety of the criticisms of Mr. Aird on a previous occasion, as to the absence of puddle, when it was said that the method of construction by basket-work, by men, women and children, was such as to knead the material into a very compact mass, so that it became of the nature of a puddle bank, the slope of the bank being two to one. In the Bann reservoirs, County Down, the slope in some cases was three to one; on an average 2½ to 1, with a puddle bank ten feet in thickness. In 1843 Mr. Thom rather curiously advocated the disuse of puddle for reservoir embankments, and described a peculiar arrangement of the laminæ of the banks. It occurred to him that the reply to the objection of Mr. Aird, that no puddle was used, viz., that the entire mass was so trodden down and consolidated as to be of the nature of puddle, might apply to some extent to the criticism of Mr. Bateman in this case.

Mr. Bateman explained that it was not the embankments he objected to, but the revetment walls, as being made too light in section.

Mr. Russell Aitken did not think any advantage had been gained by a departure from the old method of constructing earthen canal embankments, viz., a core of solid retentive earth, with a puddle trench in the center of the bank to stop any leakage. Even in England there were serious objections to facing the slopes of embankments with puddle; and in India especially it was highly dangerous, because when the puddle was not covered by water, the heat of the sun dried it up, and caused it to crack to a great depth. In India, too, vermin were a source of great annoyance and danger, and one bank of which he had charge was perforated by land crabs. Indeed it was almost impossible to keep vermin out without masonry when the puddle was near the surface of the bank. They forced their way through the loose stones, and he had no doubt the banks of the Soonkesala canal would one day give a good deal of trouble from this

cause. True, he had while in India successfully repaired one embankment by placing a puddle apron on the face of the embankment; but in order to prevent the heat of the sun and vermin injuring the puddle, he had covered it with pitching in Portland cement, through which vermin could not penetrate. The embankment was watered once a day to prevent it cracking.

He had some time since looked over Sir Arthur Cotton's writings, and he thought that officer's views would be very well represented by the following extract from one of them: "Several lines of railway have sealed up their tract of country probably for ever against cheap transit, and even against irrigation, for the railway men, must as it were for their lives resist irrigation works in the neighborhood of their lines, as inevitably leading to cheap transit. . . . . If the thirty millions to be spent in railways become a real warning to us, and make us consider, whether proposed works are really suited to the circumstances of India or not, they will not have been spent in vain. . . . . Every shareholder, and every person in any way committed to that dreadful mistake is now, in theory at least, a necessary opponent of the works that are essential to the material progress of India." That opinion did not agree with the North-Western Provinces Public Works Report for 1869-70, when, without the Ganges canal and other irrigation canals, there would have been a famine. It was there laid down that the irrigation canals by producing, and the railways by carrying the produce, warded off a famine from Northern India. He believed Sir Arthur Cotton overrated the advantages of canals as a means of communication. The Ganges canal, although constructed for and used by barges, was almost worthless as a means of transit; nor was the revenue derived from tonnage dues of any material importance.

It was stated in the Paper that 50 per cent. of the cost of the works was spent on engineering and for unused stores. That seemed to be a very large percentage indeed, and he could only attribute it to the fact, that the money expended on these works was capital guaranteed by the Government of India. He had obtained from Mr. Fleming, who conducted the affairs of the Elphinstone Reclamation Company in Bombay, an unguaranteed work, to a successful termination, a return showing the cost of engineering during the construction of those works, from which he found that when, as in 1866, the expenditure was £350,000 for the year, the engineering cost 2½ per cent.; afterwards it was, on £280,000, 3 per cent.; on £140,000, 6¼ per cent.; on £180,000, 4⅜ per cent.; and in 1870, on £100,000, it was 7 per cent. In 1868, Mr. Aitken reported that the cost of engineering many miscellaneous works in the town and island of Bombay, aggregating several hundred thousand pounds, was 4.8 per cent. on the expenditure. In the Public Works Department, which was burdened with a number of persons who were not available for strictly engineering work, the cost of engineering was understood to be 25 or 30 per cent. on the money expended. He therefore thought some explanation on this point was due; for, even allowing, £500,000 to have been expended on engineering and unused stores, there was still a sum of about £250,000 unaccounted for, and available for cost of management, &c.

If there was no error in the paper, it condemned most effectually the guarantee system when applied to canals, which ought to be treated as remunerative works. Doubtless the guarantee system, when applied to railways, had enabled a great deal of work to be done rapidly; but railways were required for political and military purposes, and could not in India, up to this time, be considered simply as remunerative works.

Mr. R. Rawlinson, C.B., said he could not tell whether the diagrams of reservoir embankments exhibited were for instruction or for warning. If for instruction, he thought they were calculated to teach a very bad lesson; if for warning he would point out a few of their mal-arrangements. Looking at the diagrams, they seemed to represent ordinary cross sections of a waterworks impounding embankment; having slopes of three to one on the inside and of about two to one on the outside. No puddle trench was shown below the surface, nor any puddle wall in the main portion of the bank. The outside half of the embankment was of porous ma-

terial—purposely, as it seemed: the inner portion he assumed was intended to be water-tight, or, at all events, to be made water-tight when protected by puddle on the surface. He cautioned young engineers against following such an example as that; for a lining of puddle laid on the surface was most troublesome, especially if it lay on a mass of earth liable to subsidence and motion such as water-works banks would have, and especially if the material on which it rested was in any degree porous. Now, although he did not in all respects agree with the modern practice of sinking a puddle trench to a great depth (150 feet in a recent case) to get to what was supposed to be a water-tight stratum, yet he held that the practice of sinking a puddle trench to a moderate depth was one which might be safely followed; and he also held that the modern practice of constructing a puddle wall in the center of the bank had some advantages if the puddle wall was supported by carefully selected material, that was, by a pyramidal core of water-tight material. The tail of the outer half of the bank might then be of porous material, and even the inner foot of the bank also, the core being sound and water-tight; but to him a porous bank in the way shown on the sections, either for first construction or subsequent repair, was, in his opinion, very defective engineering, and might prove to be a very misleading, because dangerous, operation. If the inner slope was of a material not in itself water-tight, there was a method to make it so far better than by putting puddle on its surface. If, when the reservoir was being filled with water, and as the water gradually rose against the bank, a number of men were placed along the water line with flat bars or grafting tools, and were instructed to run them down well into the material, and shake them so as to wash in any loam with the water as it rose, that would make the surface non-porous to the depth the operation was carried down, and such process would be less costly than puddle; the sun would not cause cracks, nor would vermin have the same facilities for getting through it as through puddle.* He agreed with the Author that it was better to lay broken rubble on the inner slope in preference to masonry pitching. He thought masonry pitching, or wall stone, or block-in-course, formed a dangerous coating for the inner face of a waterworks bank, as the stones were liable to jam and make an arch beneath which cavities might form, and the mischief go on for a time till a failure occurred without the engineer knowing it. When a puddle trench was in rock or stable material, it was sometimes the custom to cut vertical steps. He cautioned persons who had puddle trenches to construct never to make anything of the kind, because of the liability of subsidence of the bank. If a tangent line of the subsidence was drawn, and the lines then brought down at right angles, it would be seen that the tendency would be to draw the vertical line from the upright face. Much mischief had been caused through the drawing away of the puddle from the vertical face. It was therefore expedient to let the puddle trench rest upon a slope not much dissimilar to the slope of the side of the hill where the work was. Supposing the site of the reservoir was not absolutely water-tight, instead of sinking a puddle trench 150 or 160 feet, he would go down, say thirty feet, and would put a thick bed of concrete at the bottom and up the outer side, and then form drains from the outer face of concrete away from the trench into a manhole, where the water that came through could be seen; so that if there was leakage it would be known that it was leakage through the substratum in contact with the concrete, and would come to daylight at the foot of the slope, and could be sent into the gauge basins for compensation without causing mischief to the bank. The engineer must exercise his ingenuity as to when and how he would do these things. Under any circumstances the outer half of the embankment, commencing from the puddle trench, should be perfectly drained in a way that the drains should remain per-

* The mass of an embankment to impound and confine water under pressure should be sound and perfectly water-tight without the aid of puddle-lining, or surface washing and wraping, as described, or risk of a catastrophe would be incurred. A little care in forming a reservoir embankment is of more value than a large expenditure in subsequent repairs to patch leaks, which, as a rule, cannot be patched. The three rules in reservoir works may be thus stated—care in selecting a site, in placing the material, and in filling and subsequent working.—R. R.

manent, so that the engineer should have the opportunity of seeing any water coming from that portion of the subsoil of the bank, and if the water did not come away muddy he might rest satisfied it was not doing his bank any harm. If after repairs to the bank were necessary in the line of the puddle wall, he recommended engineers not to make the repairs good with puddle, supposing it was a soft bottom which caused leakage, but to use concrete.

Mr. E. A. Cowper asked how the clay was to be got into the bank? Mr. Rawlinson had suggested a means of dibbling in clay as the water rose, but he did not see how the clay was to be mixed by the grafting tool.

Mr. Rawlinson replied that clay was not necessary. Supposing there was a gravel or sand substratum to be made water-tight, loam or soil incorporated with the gravel would make a sort of concrete and would render the bank water-tight. It was not an idea which originated with himself, but was practised by early canal engineers, as on the Bridgewater canal. He had made a service reservoir, situated on sand, perfectly water-tight by the process of washing in loam with flat bars as described.

Mr. Beardmore said he had known several banks in which no puddle was used; for instance, the North Level seabanks were said to be chiefly composed of sand. If there was liability to rapid lowering of the water in the Soonkesala canal the puddle slopes would be liable to slip, and in that respect a center puddle wall was to be preferred.

Lieut. Col. J. G. Medley, R. E., asked whether the system suggested by Mr. Rawlinson would do for running water, or did it only apply to reservoirs or still-water canals. Such puddle banks in Upper India would have to be made in some cases where there was a velocity of water of three or four miles an hour, where, as fast as the dirt was thrown down, it would be washed away.

Mr. Rawlinson said it was self-evident that during the operation the water must be comparatively still, and there must be a sufficiency of water and no more.

Mr. R. E. Forrest said, although no one had greater respect and veneration for Sir Arthur Cotton's works on India than he had, yet he thought the views put forward with regard to canals might do great damage to the cause of irrigation. It was foolish to compare canals with railways as means of transit and carriage. Irrigation was absolutely needed; but Sir A. Cotton wanted to combine navigation with irrigation. Now it was difficult to combine the two, the requirements being so different. A navigable canal required a sluggish flow of water; but for an irrigation canal the faster the current flowed the better. A high velocity saved cost and gave a better irrigating machine. The ordinary problem that presented itself was two big rivers with a tract of land between them; and the water had to be taken from one of those rivers to irrigate that tract. The best line for carrying the canal was on the ridge between the two rivers, because it then commanded the land on each side. But this central ridge was generally the poorest and least populated, and that was the reason why an irrigation canal was wanted there. All the large cities lay along the big rivers, and to carry goods canals must be made from city to city. Therefore at starting it was found the irrigation canal run where there was no traffic; in fact, did not go near the large markets.

The slightest change in the section of a canal of the usual size in India made such a difference in the cost as to affect the whole question of the returns. For a canal, which he projected a short time since, he was told that to enable boats to stem the current the velocity must not exceed $2\frac{1}{2}$ feet per second. Supposing the current had a velocity of $3\frac{1}{2}$ feet per second, to make it navigable it would be necessary to change the whole design, and the cost would be nearly doubled. In his opinion it was preferable to keep the two questions of irrigation and navigation entirely distinct; and as food was the first necessity for the resident population, he thought it would be better to construct irrigation canals at once, leaving the consideration of the facilities to be provided for its transport to more distant localities, whether by navigable canals or otherwise, to be dealt with hereafter.

The system of canals in India was not generally understood, but it was very simple. The canal started from a big

river to irrigate a tract of land, with the water running at as high a velocity as the bed would stand. It was necessary to design the canal to carry 3,000 cubic feet of water per second in the monsoons, and only about 1,800 feet in the winter season. When in addition arrangements for navigation had to be made, it swelled the cost to such an extent as to make the returns almost problematical. He spoke from experience of some large canals which had been projected by European engineers. Without the arrangements for navigation, such as locks and still-water reaches, and as irrigation canals alone, they were calculated to give a return of 8 or 9 per cent., but when arranged for navigation it would be rather difficult to make them pay 4 per cent.

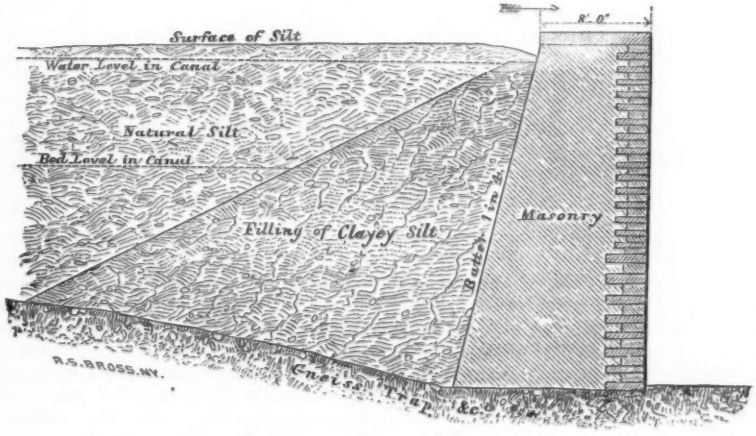
Considering that it was so difficult to get money for irrigation works, that in India there was a disinclination to raise more money by additional taxation, and that railways were better adapted for carrying than any other means of conveyance, he thought it would be better to make the irrigation canals, and to leave the navigation alone.

Mr. J. H. Latham, in reply upon the discussion, said, with regard to the face walls, Fig. 8, that there were 5 feet counterforts, sixteen feet apart, from center to center, on the water side of the wall. Considered as a retaining wall, it was stronger than many to the west and south of London, but not so strong as would be built were the sleepers of the permanent way laid close behind the coping. The gravel was not water-worn, but quartz split into irregular crystals, and was much rougher than water-worn gravel.

The masonry revetment, Fig. 9, was not lying on clear sand, as had been supposed; it was silty river sand, and a section would stand vertically twelve feet high when dry; but water would percolate through it. The puddle at the toe ran across the whole of the canal bed, and so made it water-tight, and prevented any percolation. In the flood of August, 1870, when twelve inches of rain fell in twelve hours, the water topped the masonry revetment, and it was then not a question whether the wall was a sound one or not, for the sandy bank behind must have been washed away. The revetment wall, which was not a self-supporting wall, but rested upon the bank, of course failed; but the face-wall (Fig. 8) had never suffered.

The Soonkesala anicut (Fig. 4) was generally upon solid gneiss, and occa-

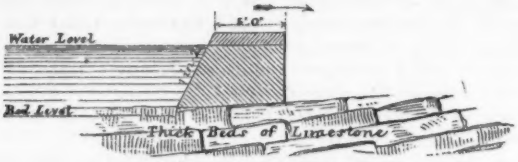
FIG. 4.



SOONKESALA WEIR, COMPLETED 1869

sionally trap; but hitherto there had been no danger from the rock being carried away. The Rajôli anicut (Fig. 6) was upon limestone, about ten inches thick in the bed; and though limestone was a treacherous formation when beds

FIG. 6.

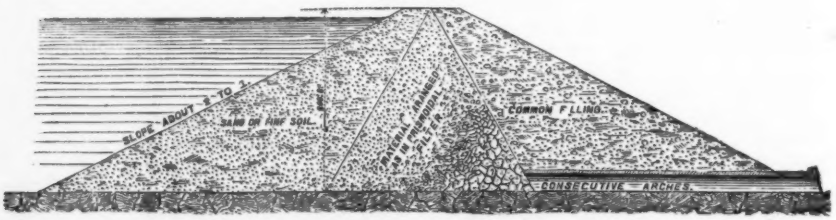


RAJOLI WEIR, COMPLETED 1869.

of dirt lay between the strata of stone, in this case the stone was solid, and, as the fall was not great, it was uninjured. The Jutoor anicut (Fig. 5) was in a part of the river that contracted below the weir, and consequently, before there was much water flowing over, the crest of the weir was almost 'drowned.' The fact that the water-cushion had not suffered therefore proved little. Indeed, the spoil, or rocky deposit of the river, rose in rear of the water-cushion, and when the water ceased flowing over the weir the standing water almost concealed the crest of the lower wall of the water-cushion. Nothing with regard to water-cushions could be learnt from that. He supposed, from the fact of the face-wall (Fig. 8) not having given way under the great flood, that the work was good. Great care was taken with it, and it was built under close supervision and inspection over very short lengths at a time.

As to the imaginary sections, he considered it an advantage to have puddle in front, where it could be got at, not quite on the slope, but protected in front. Care had to be taken of the quality of the puddle. If it was liable to run it could be put farther into the bank, and a greater weight of gravel and shingle laid in front to hold it up. But the puddle was not the only protection; it lay upon earth; and a sun-crack running through the puddle would not be liable to run continuously into the earth, which was not homogeneous with it. Even if leakage existed, the bank might theoretically be constructed so as to be secure, as in Fig. 14. In practice such

FIG. 14.



THEORETICAL BANK ON STRONG IMPERVIOUS GROUND.

a delicate arrangement of stone and sand would be difficult to obtain, but with puddle in front the leakage was so small that such refined selection was not necessary. In cases of rock foundation, or of irregular foundations of different kinds of strata, the subject was too wide to enter upon. But he had shown a puddle trench, with sheet piling in the center (Fig. 17); the object being to throw down the leakage, so that whatever there should be must flow out clear of the bank, without any tendency to scour or remove the material of the bank itself, or the material on which the rear of the bank rested.

The relative merits of railways and canals in America and India were very different. In the former case railways had been used for opening out the country; but in India canals were more suitable. In America every man understood a good deal about the repairs of railways. In almost every hamlet the first thing erected was a sawmill, and in the larger towns wooden lattice girders could be bought ready for use. In India the people knew little or nothing

about railways; but they did understand irrigation. In every village there were a number of woddahs, or native navigators, and in every district a large expenditure was incurred annually in repairing tanks, wells, &c. While in America the country was opened out by railways before it was inhabited, in India the first thing was to provide some means of irrigation, if it were only by wells: but after the country had been opened out by canals, railways would be necessary for developing trade. He believed that if a tract of country were opened out by a canal it would often be found the cheapest means of commencing a traffic to make that canal navigable; and he thought that this course had been wisely pursued in the canal under discussion. It had a series of up-takes from different rivers; and consequently never got so far from the large towns, which were generally near the river side, as the Ganges canal. It was the reverse in another point: the Ganges main canal had a fall of about eighteen inches a mile, and the distributing channels less fall; and thus, when the distributing channels were full, there was a current of three miles an hour: whereas in the Soonkesala canal the usual fall was six inches and under; that of the channels was two, four, or six feet a mile; and there was scarcely a mile and a half in any portion where the current at all approached the velocity of three miles an hour. In the Soonkesala canal the chief supply of water for irrigation was taken off from the canal just above the first drop-lock in each branch; so that if irrigation were in full action, the navigation down to the locks, and of course in the ponds between the locks, must be complete. This was not the case in the Ganges canal. The banks of the canal almost necessarily ran across some rather deep hollows. An investigation was made of the relative cost, and additional length, of running round some of the valleys instead of across them, and the balance was struck. In one case, where the circuit of the valley was made, one mile was thereby lengthened into three miles. In other cases the ratio of increase would be greater, so that it was best to carry the canal across these valleys. There were no masonry falls in the canal, and the scouring action of the under sluices in the weirs was local. In more recent works the head sluice was at right angles to, and close upon, the under sluice, the latter being three feet lower, so that the scour took place across the whole face of the head sluice. The sand in the Soonkesala anicut for almost the whole length lay within about eighteen inches of the crest.

No attempt had been made to deliver water by measurement; and though he had taken two cubic yards per hour from the Madras Government as the datum on which to calculate the capacity of irrigation, he had no more faith in that than he had in the previous Madras practice as regarded provision for drainage, which turned out fallacious. There had been no experiments to indicate how much water per hour per acre was required in that part of India. In the consideration of works of irrigation in Jamaica in 1871, the Surveyor General of Jamaica, after careful inquiry, adopted one cubic yard of water per hour for rice irrigation; he imagined that was enough for a rice crop, and that two cubic yards an hour would be too much.

The cost of the engineering staff on these works included that of the surveys for the Nellore part of the works, the Bellary scheme, and the reservoirs; also stores sufficient for executing the whole of these works; so that it was an exceptionally large percentage. But there were no contractors; the engineers themselves did the work which was usually done by contractors; and he did not think engineers would be content to carry out works of this kind at 4 or 5 per cent., including the cost of all the subordinate staff, and of both Home and Indian establishments.

GREAT inconvenience continues to be felt in consequence of the want of passenger communication between the Great Northern and Great Eastern systems at Hertford. At present persons studying Bradshaw are misled by that apparent but unreal connection of lines in one station, which consists of the great difference of one mile and a half from terminus to terminus. So travelers arrive at a full stop, suddenly, with vexation.

THE ASCENT OF STEEP RAILWAY GRADIENTS BY LOCOMOTIVES.

From the London "Mining Journal."

An interesting and valuable series of experiments has just been carried out on the High Peak Railway, of the London and North Western Company, in Derbyshire. The object was to test the practicability of working heavy traffic on steep gradients by Mr. Henry Handyside's method of conducting the traffic up steep inclines by locomotives, instead of by stationary engines. The inventor for many years held the office of Assistant Provincial Engineer to the Government of Nelson, New Zealand. The conditions of the development of this enterprising colony impressed him with the necessity of providing railways at a greatly less cost than is required to construct the even tracts which are insisted on for the main lines of this country, and the first condition clearly was to be able to work the traffic with considerably steeper gradients. Sacrificing the speed of expresses at sixty miles an hour, and being content with a more moderate though still quick rate of transit, and possessing the means of ascending and descending inclines with safety, with slight interruption to the speed of the journey, and without any serious addition to the cost, it is evident that the field of the locomotive and the railway, might be enormously extended. They might, therefore, with these conditions fulfilled, traverse districts which are very hilly, and where a gradient of one in seventy or eighty would mean an enormous cost of construction, could profitably be introduced into parts of the country where the trade and the population would not give a dividend on the cost of an ordinary railway, and could particularly be rendered available to convey mineral products, as stone, lime, &c., from the mountainous parts of the country, where they are generally found. Our main lines of railway have been made, but the feeders have yet to be constructed, and these wait only for the proof that they can be built at a low cost, on which the smaller traffic would suffice to pay an adequate dividend. In countries whose population is small, and whose export productions are mainly agricultural, such cheap railway systems would widen the area of profitable cultivation, and render natural resources available which are now worthless for want of the means of transit. It is the soberest of estimates to say that, let it be once shown that good working railways can be laid down at 4000*l.* or 5000*l.* a mile, a few years would see the length of railway lines in the world doubled.

This is the problem which Mr. Handyside has set himself to solve, convinced that all existing methods are essentially imperfect. The distinguishing feature of his sytem is to fix the locomotive to the rails; and, for the ascent of the incline, to convert it into a stationary engine. His plan was explained in a paper read at the meeting of the British Association at Bristol last year, and published as a pamphlet, and was for several weeks at work in the conveyance of material out of the basin of the new Avonmouth Dock at Bristol. The construction is simple. At the back of the locomotive a horizontal drum is fixed, round which a steel wire-rope is coiled. The drum is rotated by separate cylinders, and in the one used in the experiments to be described, the revolution was produced internally by what is known as the sun and planet motion, but this is no necessary part of the invention.

On each side of the engine-framing is suspended a self-acting gripping strut. These, when pressed by levers, clasp the sides of the rail firmly, and so fix the engine in its position and render it stationary. The engine is of light construction, which involves a saving in the weight to be moved, and in the weight of rail and general construction of the permanent way; but the essential novelty is the use of an effective brake to give great power in arresting speed in the descent, and particularly in fixing the engine in the ascent, then turning the steam into the cylinders which produce the revolution of the drum, and

drawing up the load by the steel wire-rope. The new method has been successfully tested by models, and by its practical application in hauling at the Avonmouth Docks, at the latter on a gradient of one in ten. But what was needed to prove its complete applicability to railway work, was that it should perform the actual traffic of a line of railway.

A specially favorable opportunity for the application of this test has been placed at the disposal of the inventor and his partners in the enterprise, by the courtesy of the London and North-Western Railway Company. They are the proprietors of the High Peak Railway. This, as its name indicates, traverses the summits of the ridge which forms the western watershed of the Derwent and the Wye, and separates their valleys from that of the Dove. It starts from Cromford, on the right bank of the Derwent, not far from Ambergate. It rises to the foot of the Black Rock by a gradient of one in eight, and passing over the road from Wirksworth to Cromford, again ascends an incline to Middleton of one in ten. It proceeds from that point on the level to the foot of the Hopton incline, which is one-quarter of a mile long, and has a gradient of one in fourteen. Thence there is a long stretch of twelve miles on the summit of the hills to the incline at Shallcross, which has a gradient of one in ten or eleven. The line joins the North-Western Railway proper beyond Buxton. This is one of the oldest railways in the kingdom, having been constructed in 1826 as a horse tramway, but it has for some years been worked by locomotives, with stationary engines for the inclines. In addition to the main line there are several sidings. The line is single, and does not carry passengers, the principal freights being limestone, the fine Hopton paving stone got near Wirksworth, and other building stones. The traffic on the line is considerable, though it is not regarded as a profitable part of the London and North-Western system.

The incline chosen to test the power of Mr. Handyside's system is that at Hopton, at the head of the famous Via Gellia. It is ordinarily worked by the balance system, which is convenient at a colliery or a quarry on an eminence, where full trucks go down and empty ones up, but as part of the line for a miscellaneous traffic it often involves a good deal of waiting to secure a down-load to balance the up-load. A long three-quarter inch endless chain, passing over iron pulleys, derives its motion from the drum attached to a double stationary engine of great power placed on the top of the incline. The wear is evidently heavy, and the weight and friction of the long chain add greatly to the work of the engine. For the last three weeks Mr. Handyside, with his brother-in-law, Major Haverfield, have been superintending the working of the traffic by the new system, which has been done with perfect ease. The London and North-Western Company have had time-keepers to note the work done and the time occupied, and one of their principal engineers, Mr. Footner, has carefully watched the working of the new system. On Thursday, after the ordinary day's work had been done, some hours were devoted to experimental trips with successful results, but we prefer describing what we saw and noted on Friday. A line of light rails, fifty-four pounds to the yard, has been laid on unballasted sleepers without chairs. The head of the rails is thicker than usual, but the shank and foot are light, and the rail is of course not reversible. The work does not appear in the least to have disturbed or injured the rails.

The mode of ascending the incline was as follows :—The engine was coupled at the siding to three trucks loaded with limestone or iron chairs or rails, and weighing without the engine, which is twenty-two tons, about thirty-five tons. The train ran along a quarter of a mile of level line to the foot of the incline. Here, without any loss of time, the engine was uncoupled, the trucks remained stationary, and the engine ran up the incline at the rate of about twenty-five miles an hour, paying out the steel wire rope from the drum, that having been previously fastened to the front truck. Having ascended half way the engine stopped, the grippers were dropped on and clasped the rails, and the steam was turned into the cylinders which worked the drum. The three trucks were then

rapidly drawn up by the rope, and as soon as they reached the engine a strut at the rear was dropped, which firmly supported them, the engine ran to the top of the incline, was again fixed to the rails, and drew the trucks up to the top, was coupled to them, and drew them away to a siding, and then ran down the incline without any necessity for the application of the brake grippers, fetched three more trucks, and repeated the operation.

The special timed work done on Friday, was to convey eighteen trucks loaded with stone and iron, and weighed by the London and North-Western Company, from the Middleton siding to the top of the Hopton incline. The whole weight without the engine was 190 tons. The engine reached the loaded trucks at the siding at thirty-four minutes forty seconds after one o'clock. The trucks were taken up three at a time, and the whole eighteen were raised to the top of the incline, and all but the last shunted into a siding, at thirteen minutes fifteen seconds after three o'clock, so that the whole work occupied one hour thirty-eight minutes, thirty-five seconds. Of this time, however, seventeen minutes forty-three seconds were taken up by traversing the level part of the line to the foot of the incline, which formed no part of the work to be done as compared with that of a stationary engine. Again the men stopped to take in water, from not having first filled the boiler, and that occupied six minutes fifty-seven seconds. Deducting these two items, the actual time taken to raise 190 tons up an incline a quarter of a mile long, on a gradient of one in fourteen, and, therefore, rising 100 feet, was one hour thirteen minutes fifty-five seconds. The time was thus occupied :

	Hrs. ms. scs.
Traversing level line	0 17 43
Ascending incline	0 40 48
Descending incline	0 8 33
Shunting at top	0 16 59
Coupling at foot and once shunting to clear a siding	0 7 35
Taking in water	0 6 57
Total	1 38 35

From what we know of the working of a stationary engine, which only raises one loaded truck at once, we believe the work was done in less time than that would occupy without deductions, but in that case there must have been a locomotive at the foot to drag the trucks to the incline, and one at the top to shunt and to carry them forward. In the case of the locomotive tried on Friday, it did all the work itself, and the train was shunted, ready to be coupled to the last three trucks, and proceeded on its journey. The average time occupied in each ascent was rather under six minutes five seconds, and the engine only once took over a minute and a half to go down, that being when the driver stopped to grease. Two of the descents were made in a minute and a quarter each, and there was no necessity to apply the brake. The engine previously took seven loaded trucks down to complete the load at the siding, four at one descent and three at another. The brakes were on the trucks, but it was not necessary to apply the engine brake, and they went down quite steadily. Supposing that the engine had been drawing a load of three trucks, weighing, as one lot of these did, thirty-seven and a half tons, not having to separate them it would only have occupied six minutes and five seconds in the ascent of 100 feet, and in traversing a quarter of a mile, which would have been an almost unappreciable interruption for the incline. Had the load been six trucks, the time, with once coupling and shunting, would not have exceeded sixteen and a half minutes, and supposing the ordinary speed on the level to be at the rate of twenty miles an hour, the loss of time would be a quarter of an hour for the incline, as compared with a perfectly level surface. A light passenger train, such as would traverse a line of this character, would not require division, so that the interruption on ascending the incline would be very slight.

On Saturday, the engine took a train of twenty-seven loaded trucks, weighing 280 tons, from the top of the Hopton incline to Hurdlow—a distance of twelve miles, stopping at five places and shunting on the way. It traversed the whole distance in two hours fifty-three minutes, consuming 500 gallons of water and five cwts. of coal. It took twelve wagons, ten of which were loaded with a total weight of 120 tons, up the incline at Hurdlow, which is one and three-quarter

mile long, and has a gradient of one in sixty, and made the ascent easily without the rope.

It descended the incline and got back to Hopton in three hours fifty-five minutes from the time it had started from the latter place. It was then supplied with water, went down the incline, fetched up seven loaded wagons in two draws, waited for the down train, and took that train of nine wagons, weighing ninety tons, at once down the incline and on to Middleton.

We have stated the facts with as little comment as possible, and they go far to prove that Mr. Handyside has shown the railway world how to construct cheap railways, and he is hardly extravagant in stating that with his mode of traversing inclines, railways may be made in any part of the country at £4,000 a mile. If that be so, the area of steam locomotion may be indefinitely extended, and the traffic on the main lines increased by the construction of numerous feeders across districts which would not pay for an expensive even-graded line.

This method of ascending and descending inclines may prove of great advantage in warlike operations, and the War Office so thoroughly appreciates the importance of the experiments that the Royal Engineer Committee on Field Railways will visit Hopton to report on the system early next month.

THE ORIGIN AND MEANING OF GEOMETRICAL AXIOMS.

By H. HELMHOLTZ.

From "Mind."

My object in this article is to discuss the philosophical bearing of recent inquiries concerning geometrical axioms and the possibility of working out analytically other systems of geometry with other axioms than Euclid's. The original works on the subject, addressed to experts only, are particularly abstruse, but I will try to make it plain even to those who are not mathematicians. It is of course no part of my plan to prove the new doctrines correct as mathematical conclusions. Such proof must be sought in the original works themselves.

Among the first elementary propositions of geometry, from which the student is led on by continuous chains of reasoning to the laws of more and more complex figures, are some which are held not to admit of proof, though sure to be granted by every one who understands their meaning. These are the so-called Axioms; for example, the proposition that if the shortest line drawn between two points is called straight there can be only one such straight line. Again, it is an axiom that through any three points in space, not lying in a straight line, a plane may be drawn, *i.e.*, a surface which will wholly include every straight line joining any two of its points. Another axiom, about which there has been much discussion, affirms that through a point lying without a straight line only one straight line can be drawn parallel to the first; two straight lines that lie in the same plane and never meet, however far they may be produced, being called parallel. There are also axioms that determine the number of dimensions of space and its surfaces, lines and points, showing how they are continuous; as in the propositions, that a solid is bounded by a surface, a surface by a line and a line by a point, that the point is indivisible, that by the movement of a point a line is described, by that of a line a line or a surface, by that of a surface a surface or a solid, but by the movement of a solid a solid and nothing else is described.

Now what is the origin of such propositions, unquestionably true yet incapable of proof in a science where everything else is reasoned conclusion? Are they inherited from the divine source of our reason as the idealistic philosophers think, or is it only that the ingenuity of mathematicians has hitherto not been penetrating enough to find the proof?

Every new votary, coming with fresh zeal to geometry, naturally strives to succeed where all before him have failed. And it is quite right that each should make the trial afresh; for, as the question has hitherto stood, it is only by the fruitlessness of one's own efforts that one can be convinced of the impossibility of finding a proof. Meanwhile solitary inquirers are always, from time to time, appearing who become so deeply entangled in complicated trains of reasoning that they can no longer discover their mistakes and believe they have solved the problem. The axiom of parallels especially has called forth a great number of seeming demonstrations.

The main difficulty in these inquiries is and always has been the readiness with which results of every-day experience become mixed up as apparent necessities of thought with the logical processes, so long as Euclid's method of constructive intuition is exclusively followed in geometry. In particular it is extremely difficult, on this method, to be quite sure that in the steps prescribed for the demonstration we have not involuntarily and unconsciously drawn in some most general results of experience, which the power of executing certain parts of the operation has already taught us practically. In drawing any subsidiary line for the sake of his demonstration, the well-trained geometer asks always if it is possible to draw such a line. It is notorious that problems of construction play an essential part in the system of geometry. At first sight, these appear to be practical operations, introduced for the training of learners; but in reality they have the force of existential propositions. They declare that points, straight lines or circles, such as the problem requires to be constructed, are possible under all conditions, or they determine any exceptions that there may be. The point on which the investigations turn that we are going to consider is essentially of this nature. The foundation of all proof by Euclid's method consists in establishing the congruence of lines, angles, plane figures, solids, &c. To make the congruence evident, the geometrical figures are supposed to be applied to one another, of course without changing their form and dimensions. That this is in fact possible we have all experienced from our earliest youth. But, when we would build necessities of thought upon this assumption of the free translation of fixed figures with unchanged form to every part of space, we must see whether the assumption does not involve some presupposition of which no logical proof is given. We shall see later on that it does contain one of most serious import. But if so, every proof by congruence rests upon a fact which is obtained from experience only.

I offer these remarks at first only to show what difficulties attend the complete analysis of the presuppositions we make in employing the common constructive method. We evade them when we apply to the investigation of principles the analytical method of modern algebraical geometry. The whole process of algebraical calculation is a purely logical operation; it can yield no relation between the quantities submitted to it that is not already contained in the equations which give occasion for its being applied. The recent investigations have accordingly been conducted almost exclusively by means of the purely abstract methods of analytical geometry.

However, after discovering by the abstract method what are the points in question, we shall best get a distinct view of them by taking a region of narrower limits than our own world of space. Let us, as we logically may, suppose reasoning beings of only two dimensions to live and move on the surface of some solid body. We will assume that they have not the power of perceiving anything outside this surface, but that upon it they have perceptions similar to ours. If such beings worked out a geometry, they would of course assign only two dimensions to their space. They would ascertain that a point in moving describes a line, and that a line in moving describes a surface. But they could as little represent to themselves what further spatial construction would be generated by a surface moving out of itself, as we can represent what would be generated by a solid moving out of the space we know. By the much abused expression "to represent" or "to be able to think how something happens" I understand—and I do not see how anything else can be understood by it with-

out loss of all meaning—the power of imagining the whole series of sensible impressions that would be had in such a case. Now as no sensible impression is known relating to such an unheard-of event as the movement to a fourth dimension would be to us, or as a movement to our third dimension would be to the inhabitants of a surface, such a "representation" is as impossible as the "representation" of colors would be to one born blind, though a description of them in general terms might be given to him.

Our surface-beings would also be able to draw shortest lines in their superficial space. These would not necessarily be straight lines in our sense, but what are technically called *geodetic* lines of the surface on which they live, lines such as are described by a tense thread laid along the surface and which can slide upon it freely. I will henceforth speak of such lines as the *straightest* lines of any particular surface or given space, so as to bring out their analogy with the straight line in a plane.

Now if beings of this kind lived on an infinite plane, their geometry would be exactly the same as our planimetry. They would affirm that only one straight line is possible between two points, that through a third point lying without this line only one line can be drawn parallel to it, that the ends of a straight line never meet though it is produced to infinity, and so on. Their space might be infinitely extended, but even if there were limits to their movement and perception, they would be able to represent to themselves a continuation beyond these limits, and thus their space would appear to them infinitely extended, just as ours does to us, although our bodies cannot leave the earth and our sight only reaches as far as the visible fixed stars.

But intelligent beings of the kind supposed might also live on the surface of a sphere. Their shortest or straightest line between two points would then be an arc of the great circle passing through them. Every great circle passing through two points is by these divided into two parts, and if they are unequal the shorter is certainly the shortest line on the sphere between the two points, but also the other or larger arc of the same great circle is a geodetic or straightest line, *i.e.*, every smaller part of it is the shortest line between its ends. Thus the notion of the geodetic or straightest line is not quite identical with that of the shortest line. If the two given points are the ends of a diameter of the sphere, every plane passing through this diameter cuts semicircles on the the surface of the sphere all of which are shortest lines between the ends; in which case there is an infinite number of equal shortest lines between the given points. Accordingly, the axiom of there being only one shortest line between two points would not hold without a certain exception for the dwellers on a sphere.

Of parallel lines the sphere-dwellers would know nothing. They would declare that any two straightest lines, sufficiently produced, must finally cut not in one only but in two points. The sum of the angles of a triangle would be always greater than two right angles, increasing as the surface of the triangle grew greater. They could thus have no conception of geometrical similarity between greater and smaller figures of the same kind, for with them a greater triangle must have different angles from a smaller one. Their space would be unlimited, but would be found to be finite or at least represented as such.

It is clear, then, that such beings must set up a very different system of geometrical axioms from that of the inhabitants of a plane or from ours with our space of three dimensions, though the logical powers of all were the same; nor are more examples necessary to show that geometrical axioms must vary according to the kind of space inhabited. But let us proceed still farther.

Let us think of reasoning beings existing on the surface of an egg-shaped body. Shortest lines could be drawn between three points of such a surface and a triangle constructed. But if the attempt were made to construct congruent triangles at different parts of the surface, it would be found that two triangles with three pairs of equal sides would not have their angles equal. The sum of the angles of a triangle drawn at the sharper pole of the body would depart farther from two right angles than if the triangle were drawn at the blunter pole or at the equator. Hence it appears

that not even such a simple figure as a triangle can be moved on such a surface without change of form. It would also be found that if circles of equal radii were constructed at different parts of such a surface (the length of the radii being always measured by shortest lines along the surface) the periphery would be greater at the blunter than at the sharper end.

We see accordingly that, if a surface admits of the figures lying on it being freely moved without change of any of their lines and angles as measured along it, the property is a special one and does not belong to every kind of surface. The condition under which a surface possesses this important property was pointed out by Gauss in his celebrated treatise on the curvature of surfaces. The "measure of curvature," as he called it, *i.e.*, the reciprocal of the product of the greatest and least radii of curvature, must be everywhere equal over the whole extent of the surface.

Gauss showed at the same time that this measure of curvature is not changed if the surface is bent without distension or contraction of any part of it. Thus we can roll up a flat sheet of paper into the form of a cylinder or of a cone without any change in the dimensions of the figures taken along the surface of the sheet. Or the hemispherical fundus of a bladder may be rolled into a spindle-shape without altering the dimensions on the surface. Geometry on a plane will therefore be the same as on a cylindrical surface; only in the latter case we must imagine that any number of layers of this surface, like the layers of a rolled sheet of paper, lie one upon another and that after each entire revolution round the cylinder a new layer is reached.

These observations are meant to give the reader a notion of a kind of surface the geometry of which is on the whole similar to that of the plane, but in which the axiom of parallels does not hold good, namely, a kind of curved surface which geometrically is, as it were, the counterpart of a sphere, and which has therefore been called the *pseudospherical surface* by the distinguished Italian mathematician, E. Beltrami, who has investigated its properties. It is a saddle-shaped surface of which only limited pieces or strips can be connectedly represented in our space, but which may yet be thought of as infinitely continued in all directions, since each piece lying at the limit of the part constructed can be conceived as drawn back to the middle of it and then continued. The piece displaced must in the process change its flexure but not its dimensions, just as happens with a sheet of paper moved about a cone formed out of a plane rolled up. Such a sheet fits the conical surface in every part, but must be more bent near the vertex and cannot be so moved over the vertex as to be at the same time adapted to the existing cone and to its imaginary continuation beyond.

Like the plane and the sphere, pseudospherical surfaces have their measure of curvature constant, so that every piece of them can be exactly applied to every other piece, and therefore all figures constructed at one place on the surface can be transferred to any other place with perfect congruity of form and perfect equality of all dimensions lying in the surface itself. The measure of curvature as laid down by Gauss, which is positive for the sphere and zero for the plane, would have a constant negative value for pseudospherical surfaces, because the two principal curvatures of a saddle-shaped surface have their concavity turned opposite ways.

A strip of a pseudospherical surface may, for example, be represented by the inner surface (turned towards the axis) of a solid anchor-ring. If the plane figure *aabb* (Fig. 1) is made to revolve

FIG. 1.

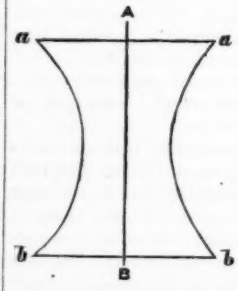


FIG. 2.

on its axis of symmetry A B, the two arcs *a b* will describe a pseudospherical concave-convex surface like that of the ring. Above and below, towards *a a* and *b b*, the surface will turn outwards with

ever-increasing flexure, till it becomes perpendicular to the axis and ends at the edge with one curvature infinite. Or, again, half of a pseudospherical surface may be rolled up into the shape of a champagne-glass (Fig. 2) with tapering stem infinitely prolonged. But the surface is always necessarily bounded by a sharp edge beyond which it cannot be directly continued. Only by supposing each single piece of the edge cut loose and drawn along the surface of the ring or glass, can it be brought to places of different flexure at which farther continuation of the piece is possible.

In this way too the straightest lines of the pseudospherical surface may be infinitely produced. They do not like those on a sphere return upon themselves, but, as on a plane, only one shortest line is possible between two given points. The axiom of parallels does not however hold good. If a straightest line is given on the surface and a point without it, a whole pencil of straightest lines may pass through the point, no one of which, though infinitely produced, cuts the first line; the pencil itself being limited by two straightest lines, one of which intersects one of the ends of the given line at an infinite distance, the other the other end.

As it happened, a system of geomety excluding the axiom of parallels was devised on Euclid's synthetic method, as far back as the year 1829, by N. J. Lobatschewsky, professor of mathematics at Kasan, and it was proved that this system could be carried out as consistently as Euclid's. It agrees exactly with the geometry of the pseudospherical surfaces worked out recently by Beltrami.

Thus we see that in the geometry of two dimensions a surface is marked out as a plane or a sphere or a pseudospherical surface by the assumption that any figure may be moved about in all directions without change of dimensions. The axiom that there is only one shortest line between any two points distinguishes the plane and the pseudospherical surface from the sphere, and the axiom of parallels marks off the plane from the pseudosphere. These three axioms are in fact necessary and sufficient to define as a plane the surface to which Euclid's planimetry has reference, as distinguished from all other modes of space in two dimensions.

The difference between plane and spherical geometry has been long evident, but the meaning of the axiom of parallels could not be understood till Gauss had developed the notion of surfaces flexible without dilatation and consequently that of the possibly infinite continuation of pseudospherical surfaces. Inhabiting a space of three dimensions and endowed with organs of sense for their perception, we can represent to ourselves the various cases in which beings on a surface might have to develop their perception of space; for we have only to limit our own perceptions to a narrower field. It is easy to think away perceptions that we have; but it is very difficult to imagine perceptions to which there is nothing analogous in our experience. When, therefore, we pass to space of three dimensions we are stopped in our power of representation by the structure of our organs and the experiences got through them which correspond only to the space in which we live.

There is however another way of treating geometry scientifically. All known space-relations are measurable, that is they may be brought to determination of magnitudes (lines, angles, surfaces, volumes). Problems in geometry can therefore be solved by finding methods of calculation for arriving at unknown magnitudes from known ones. This is done in analytical geometry, where all forms of space are treated only as quantities and determined by means of other quantities. Even the axioms themselves make reference to magnitudes. The straight line is defined as the *shortest* between two points, which is a determination of quantity. The axiom of parallels declares that if two straight lines in a plane do not intersect (are parallel), the alternate angles, or the corresponding angles, made by a third line intersecting them, are equal; or it may be laid down instead that the sum of the angles of any triangle is equal to two right angles. These are determinations of quantity.

Now we may start with this view of space, according to which the position of a point may be determined by measurements in relation to any given figure (system of co-ordinates) taken as fixed,

and then inquire what are the special characteristics of our space as manifested in the measurements that have to be made, and how it differs from other extended quantities of like variety. This path was first entered by one too early lost to science, B. Riemann of Göttingen. It has the peculiar advantage that all its operations consist in pure calculation of quantities, which quite obviates the danger of habitual perceptions being taken for necessities of thought.

The number of measurements necessary to give the position of a point is equal to the number of dimensions of the space in question. In a line the distance from one fixed point is sufficient, that is to say, one quantity; in a surface the distances from two fixed points must be given; in space, the distances from three; or we require as on the earth longitude, latitude and height above the sea, or, as is usual in analytical geometry, the distances from three co-ordinate planes. Riemann calls a system of differences, in which one thing can be determined by n measurements, an "$n$fold extended aggregate" or an "aggregate of n dimensions." Thus the space in which we live is a three-fold, a surface is a two-fold and a line is a simple extended aggregate of points. Time also is an aggregate of one dimension. The system of colors is an aggregate of three dimensions, inasmuch as each color, according to the investigations of Th. Young and Clerk Maxwell, may be represented as a mixture of three primary colors, taken in definite quantities. The particular mixtures can be actually made with the color-top.

In the same way we may consider the system of simple tones as an aggregate of two dimensions, if we distinguish only pitch and intensity and leave out of account differences of timbre. This generalisation of the idea is well-suited to bring out the distinction between space of three dimensions and other aggregates. We can, as we know from daily experience, compare the vertical distance of two points with the horizontal distance of two others, because we can apply a measure first to the one pair and then to the other. But we cannot compare the difference between two tones of equal pitch and different intensity with that between two tones of equal intensity and different pitch. Riemann showed by considerations of this kind that the essential foundation of any system of geometry is the expression that it gives for the distance between two points lying in any direction from one another, begining with the interval as infinitesimal. He took from analytical geometry the most general form for this expression, that, namely, which leaves altogether open the kind of measurements by which the position of any point is given.* Then he showed that the kind of free mobility without change of form which belongs to bodies in our space can only exist when certain quantities yielded by the calculation†—quantities that coincide with Gauss's measure of surface-curvature when they are expressed for surfaces—have everywhere an equal value. For this reason Riemann calls these quantities, when they have the same value in all directions for a particular spot, the measure of curvature of the space at this spot. To prevent misunderstanding I will once more observe that this so-called measure of space-curvature is a quantity obtained by purely analytical calculation and that its introduction involves no suggestion of relations that would have a meaning only for sense-perception. The name is merely taken, as a short expression for a complex relation, from the one case in which the quantity designated admits of sensible representation.

Now whenever the value of this measure of curvature in any space is everywhere zero, that space everywhere conforms to the axioms of Euclid; and it may be called a *flat* (*homaloid*) space in contradistinction to other spaces, analytically constructible, that may be called *curved* because their measure of curvatures has value other than zero. Analytical geometry may be as completely and consistently worked out for such spaces as ordinary geometry for our actually existing homaloid space.

If the measure of curvature is positive we have *spherical* space, in which straightest lines return upon themselves and there are no parallels. Such a space

* For the square of the distance of two infinitely near points the expression is a homogeneous quadric function of the differentials of their co-ordinates.

† They are algebraical expressions compounded from the co-efficients of the various terms in the expression for the square of the distance of two contiguous points and from their differential quotients.

would, like the surface of a sphere, be unlimited but not infinitely great. A constant negative measure of curvature on the other hand gives *pseudospherical* space, in which straightest lines run out to infinity and a pencil of straightest lines may be drawn in any flattest surface through any point which does not intersect another given straightest line in that surface.

Beltrami has rendered these last relations imaginable by showing that the points, lines and surfaces of a pseudospherical space of three dimensions can be so portrayed in the interior of a sphere in Euclid's homaloid space, that every straightest line or flattest surface of the pseudospherical space is represented by a straight line or a plane, respectively, in the sphere. The surface itself of the sphere corresponds to the infinitely distant points of the pseudospherical space; and the different parts of this space, as represented in the sphere, become smaller the nearer they lie to the spherical surface, diminishing more rapidly in the direction of the radii than in that perpendicular to them. Straight lines in the sphere which only intersect beyond its surface correspond to straightest lines of the pseudospherical space which never intersect.

Thus it appeared that space, considered as a region of measurable quantities, does not at all correspond with the most general conception of an aggregate of three dimensions, but involves also special conditions, depending on the perfectly free mobility of solid bodies without change of form to all parts of it and with all possible changes of direction, and, farther, on the special value of the measure of curvature which for our actual space equals, or at least is not distinguishable from, zero. This latter definition is given in the axioms of straight lines and parallels.

Whilst Riemann entered upon this new field from the side of the most general and fundamental questions of analytical geometry, I myself arrived at similar conclusions, partly from seeking to represent in space the system of colors, involving the comparison of one threefold extended aggregate with another, and partly from inquiries on the origin of our ocular measure for distances in the field of vision. Riemann starts by assuming the above-mentioned algebraical expression which represents in the most general form the distance between two infinitely near points, and deduces therefrom the conditions of mobility of rigid figures. I, on the other hand, starting from the observed fact that the movement of rigid figures is possible in our space, with the degree of freedom that we know, deduce the necessity of the algebraic expression taken by Riemann as an axiom. The assumptions that I had to make as the basis of the calculation were the following.

First, to make algebraical treatment possible, it must be assumed that the position of any point A can be determined, in relation to certain given figures taken as fixed bases, by measurement of some kind of magnitudes, as lines, angles between lines, angles between surfaces and so forth. The measurements necessary for determining the position of A are known as its co-ordinates. In general the number of co-ordinates necessary to the complete determination of the position of a point marks the number of the dimensions of the space in question. It is further assumed that with the movement of the point A the magnitudes used as co-ordinates vary continuously.

Secondly, the definition of a solid body, or rigid system of points, must be made in such a way as to admit of magnitudes being compared by congruence. As we must not at this stage assume any special methods for the measurement of magnitudes, our definition can, in the first instance, run only as follows: Between the co-ordinates of any two points belonging to a solid body, there must be an equation which, however the body is moved, expresses a constant spatial relation (proving at last to be the distance) between the two points, and which is the same for congruent pairs of points, that is to say, such pairs as can be made successively to coincide in space with the same fixed pair of points.

However indeterminate in appearance, this definition involves most important consequences, because with increase in the number of points the number of equations increases much more quickly than the number of co-ordinates which they determine. Five points, A, B, C, D, E give ten different pairs of points (A B,

A C, A D, A E, B C, B D, B E, C D, C E, D E) and therefore ten equations, involving in space of three dimensions fifteen variable co-ordinates. But of these fifteen six must remain arbitrary if the system of five points is to admit of free movement and rotation, and thus the ten equations can determine only nine co-ordinates as functions of the six variables. With six points we obtain fifteen equations for twelve quantities, with seven points twenty-one equations for fifteen, and so on. Now from n independent equations we can determine n contained quantities, and if we have more than n equations, the superfluous ones must be deducible from the first n. Hence it follows that the equations which subsist between the co-ordinates of each pair of points of a solid body must have a special character, seeing that, when in space of three dimensions they are satisfied for nine pairs of points as formed out of any five points, the equation for the tenth pair follows by logical consequence. Thus our assumption for the definition of solidity becomes quite sufficient to determine the kind of equations holding between the co-ordinates of two points rigidly connected.

Thirdly, the calculation must further be based on the fact of a peculiar circumstance in the movement of solid bodies, a fact so familiar to us that but for this inquiry it might never have been thought of as something that need not be. When in our space of three dimensions two points of a solid body are kept fixed, its movements are limited to rotations round the straight line connecting them. If we turn it completely round once, it again occupies exactly the position it had at first. This fact that rotation in one direction always brings a solid body back into its original position needs special mention. A system of geometry is possible without it. This is most easily seen in the geometry of a plane. Suppose that with every rotation of a plane figure its linear dimensions increased in proportion to the angle of rotation, the figure after one whole rotation through 360 degrees would no longer coincide with itself as it was originally. But any second figure that was congruent with the first in its original position might be made to coincide with it in its second position by being also turned through 360 degrees. A consistent system of geometry would be possible upon this supposition, which does not come under Riemann's formula.

On the other hand I have shown that the three assumptions taken together form a sufficient basis for the starting-point of Riemann's investigation, and thence for all his further results relating to the distinction of different spaces according to their measure of curvature.

It still remained to be seen whether the laws of motion as dependent on moving forces could also be consistently transferred to spherical or pseudospherical space. This investigation has been carried out by Professor Lipschitz of Bonn. It is found that the comprehensive expression for all the laws of dynamics, Hamilton's principle, may be directly transferred to spaces of which the measure of curvature is other than zero. Accordingly, in this respect also the disparate systems of geometry lead to no contradiction.

We have now to seek an explanation of the special characteristics of our own flat space, since it appears that they are not implied in the general notion of an extended quantity of three dimensions and of the free mobility of bounded figures therein. Necessities of thought, involved in such a conception, they are not. Let us then examine the opposite assumption as to their origin being empirical, and see if they can be inferred from facts of experience and so established, or if, when tested by experience, they are perhaps to be rejected. If they are of empirical origin we must be able to represent to ourselves connected series of facts indicating a different value for the measure of curvature from that of Euclid's flat space. But if we can imagine such spaces of other sorts, it cannot be maintained that the axioms of geometry are necessary consequences of an *à priori* transcendental form of intuition, as Kant thought.

The distinction between spherical, pseudospherical and Euclid's geometry depends, as was above observed, on the value of a certain constant called by Riemann the measure of curvature of the space in question. The value must be zero for Euclid's axioms to hold good. If it were not zero, the sum of the angles of a large triangle would

differ from that of the angles of a small one, being larger in spherical, smaller in pseudospherical space. Again, the geometrical similarity of large and small solids and figures is possible only in Euclid's space. All systems of practical mensuration that have been used for the angles of large rectilinear triangles, and especially all systems of astronomical measurement which make the parallax of the immeasurably distant fixed stars equal to zero (in pseudospherical space the parallax even of infinitely distant points would be positive), confirm empirically the axiom of parallels and show the measure of curvature of our space thus far to be indistinguishable from zero. It remains, however, a question, as Riemann observed, whether the result might not be different if we could use other than our limited base-lines, the greatest of which is the major axis of the earth's orbit.

Meanwhile, we must not forget that all geometrical measurements rest ultimately upon the principle of congruence. We measure the distance between points by applying to them the compass, rule or chain. We measure angles by bringing the divided circle or theodolite to the vertex of the angle. We also determine straight lines by the path of rays of light which in our experience is rectilinear; but that light travels in shortest lines as long as it continues in a medium of constant refraction would be equally true in space of a different measure of curvature. Thus all our geometrical measurements depend on our instruments being really, as we consider them, invariable in form, or at least on their undergoing no other than the small changes we know of as arising from variation of temperature or from gravity acting differently at different places.

In measuring we only employ the best and surest means we know of to determine what we otherwise are in the habit of making out by sight and touch or by pacing. Here our own body with its organs is the instrument we carry about in space. Now it is the hand, now the leg that serves for a compass, or the eye turning in all directions is our theodolite for measuring arcs and angles in the visual field.

Every comparative estimate of magnitudes or measurement of their spatial relations proceed therefore upon a supposition as to the behaviour of certain physical things, either the human body or other instruments employed. The supposition may be in the highest degree probable and in closest harmony with all other physical relations known to us, but yet it passes beyond the scope of pure space-intuition.

It is in fact possible to imagine conditions for bodies apparently solid such that the measurements in Euclid's space become what they would be in spherical or pseudospherical space. Let me first remind the reader that if all the linear dimensions of other bodies and our own at the same time were diminished or increased in like proportion, as for instance to half or double their size, we should with our means of space-perception be utterly unaware of the change. This would also be the case if the distension or contraction were different in different directions, provided that our own body changed in the same manner and further that a body in rotating assumed at every moment, without suffering or exerting mechanical resistance, the amount of dilatation in its different dimensions corresponding to its position at the time. Think of the image of the world in a convex mirror. The common silvered globes set up in gardens give the essential features, only distorted by some optical irregularities. A well-made convex mirror of moderate aperture represents the objects in front of it as apparently solid and in fixed positions behind its surface. But the images of the distant horizon and of the sun in the sky lie behind the mirror at a limited distance, equal to its focal length. Between these and the surface of the mirror are found the images of all the other objects before it, but the images are diminished and flattened in proportion to the distance of their objects from the mirror. The flattening, or decrease in the third dimension, is relatively greater than the decrease of the surface-dimensions. Yet every straight line or every plane in the outer world is represented by a straight line or plane in the image. The image of a man measuring with a rule a straight line from the mirror would contract more and more the farther he went, but with his shrunken rule the man in the image would count

out exactly the same number of centimeters as the real man. And, in general, all geometrical measurements of lines or angles made with regularly varying images of real instruments would yield exactly the same results as in the outer world, all congruent bodies would coincide on being applied to one another in the mirror as in the outer world, all lines of sight in the outer world would be represented by straight lines of sight in the mirror. In short I do not see how men in the mirror are to discover that their bodies are not rigid solids and their experiences good examples of the correctness of Euclid's axioms. But if they could look out upon our world as we can look into theirs, without overstepping the boundary, they must declare it to be a picture in a spherical mirror, and would speak of us just as we speak of them; and if two inhabitants of the different worlds could communicate with one another, neither, so far as I can see, would be able to convince the other that he had the true, the other the distorted relations. Indeed I cannot see that such a question would have any meaning at all so long as mechanical considerations are not mixed up with it.

Now Beltrami's representation of pseudospherical space in a sphere of Euclid's space is quite similar except that the background is not a plane as in the convex mirror, but the surface of a sphere, and that the proportion in which the images as they approach the spherical surface contract, has a different mathematical expressson. If we imagine then, conversely, that in the sphere, for the interior of which Euclid's axioms hold good, moving bodies contract as they depart from the center like the images in a convex mirror, and in such a way that their representatives in pseudospherical space retain their dimensions unchanged,—observers whose bodies were regularly subjected to the same change would obtain the same results from the geometrical measurements they could make as if they lived in pseudospherical space.

We can even go a step further, and infer how the objects in a pseudospherical world, were it possible to enter one, would appear to an observer whose eye-measure and experiences of space had been gained like ours in Euclid's space. Such an observer would continue to look upon the rays of light or the lines of vision as straight lines, such as are met with in flat space and as they really are in the spherical representation of pseudospherical space. The visual image of the objects in pseudospherical space would thus make the same impression upon him as if he were at the center of Beltrami's sphere. He would think he saw the most remote objects round about him at a finite distance,* let us suppose a hundred feet off. But as he approached these distant objects, they would dilate before him, though more in the third dimension than superficially, while behind him they would contract. He would know that his eye judged wrongly. If he saw two straight lines which in his estimate ran parallel for the hundred feet to his world's end, he would find on following them that the farther he advanced the more they diverged, because of the dilatation of all the objects to which he approached. On the other hand—behind him their distance would seem to diminish, so that as he advanced they would appear always to diverge more and more. But two straight lines which, from his first position, seemed to converge to one and the same point of the background a hundred feet distant, would continue to do this however far he went, and he would never reach their point of intersection.

Now we can obtain exactly similar images of our real world if we look through a large convex lens of corresponding negative focal length, or even through a pair of convex spectacles if ground somewhat prismatically to resemble pieces of one continuous larger lens. With these, like the convex mirror, we see remote objects as if near to us, the most remote appearing no farther distant than the focus of the lens. In going about with this lens before the eyes, we find that the objects we approach dilate exactly in the manner I have described for pseudospherical space. Now any one using a lens, were it even so strong as to have a focal length of only sixty inches, to say nothing of a hundred feet, would perhaps observe for the first moment that he saw objects brought

* The reciprocal of the square of this distance, expressed in negative quantity, would be the measure of curvature of the pseudospherical space.

nearer. But after going about a little the illusion would vanish, and in spite of the false images he would judge of the distances rightly. We have every reason to suppose that what happens in a few hours to any one beginning to wear spectacles would soon enough be experienced in pseudospherical space. In short, pseudospherical space would not seem to us very strange, comparatively speaking; we should only at first be subject to illusions in measuring by eye the size and distance of the more remote objects.

There would be illusions of an opposite description, if, with eyes practiced to measure in Euclid's space, we entered a spherical space of three dimensions. We should suppose the more distant objects to be more remote and larger than they are, and should find on approaching them that we reach them more quickly than we expected from their appearance. But we should also see before us objects that we can fixate only with diverging lines of sight, namely, all those at a greater distance from us than the quadrant of a great circle. Such an aspect of things would hardly strike us as very extraordinary, for we can have it even as things are if we place before the eye a slightly prismatic glass with the thicker side towards the nose: the eyes must then become divergent to take in distant objects. This excites a certain feeling of unwonted strain in the eyes but does not perceptibly change the appearance of the objects thus seen. The strangest sight, however, in the spherical world would be the back of our own head, in which all visual lines not stopped by other objects would meet again, and which must fill the extreme background of the whole perspective picture.

At the same time it must be noted that as a small elastic flat disc, say of india-rubber, can only be fitted to a slightly curved spherical surface with relative contraction of its border and distension of its center, so our bodies, developed in Euclid's flat space, could not pass into curved space without undergoing similar distensions and contractions of their parts, their coherence being of course maintained only in as far as their elasticity permitted their bending without breaking. The kind of distension must be the same as in passing from a small body imagined at the center of Beltrami's sphere to its pseudospherical or spherical representation. For such passage to appear possible, it will always have to be assumed that the body is sufficiently elastic and small in comparison with the real or imaginary radius of curvature of the curved space into which it is to pass.

These remarks will suffice to show the way in which we can infer from the known laws of our sensible perceptions the series of sensible impressions which a spherical or pseudospherical world would give us, if it existed. In doing so we nowhere meet with inconsistency or impossibility any more than in the calculation of its metrical proportions. We can represent to ourselves the look of a pseudospherical world in all directions just as we can develop the conception of it. Therefore it cannot be allowed that the axioms of our geometry depend on the native form of our perceptive faculty, or are in any way connected with it.

It is different with the three dimensions of space. As all our means of sense-perception extend only to space of three dimensions, and a fourth is not merely a modification of what we have but something perfectly new, we find ourselves by reason of our bodily organisation quite unable to represent a fourth dimension.

In conclusion I would again urge that the axioms of geometry are not propositions pertaining only to the pure doctrine of space. As I said before, they are concerned with quantity. We can speak of quantities only when we know of some way by which we can compare, divide and measure them. All space measurements, and therefore in general all ideas of quantities, applied to space assume the possibility of figures moving without change of form or size. It is true we are accustomed in geometry to call such figures purely geometrical solids, surfaces, angles and lines, because we abstract from all the other distinctions physical and chemical of natural bodies; but yet one physical quality, rigidity, is retained. Now we have no other mark of rigidity of bodies or figures but congruence, whenever they are applied to one another at any time or place, and after any revolution.

We cannot however decide by pure geometry and without mechanical considerations whether the coinciding bodies may not both have varied in the same sense.

If it were useful for any purpose, we might with perfect consistency look upon the space in which we live as the apparent space behind a convex mirror with its shortened and contracted background; or we might consider a bounded sphere of our space, beyond the limits of which we perceive nothing further, as infinite pseudospherical space. Only then we should have to ascribe to the bodies which appear as solid and to our own body at the same time corresponding distensions and contractions, and we must change our system of mechanical principles entirely; for even the proposition that every point in motion, if acted upon by no force, continues to move with unchanged velocity in a straight line, is not adapted to the image of the world in the convex-mirror. The path would indeed be straight, but the velocity would depend upon the place.

Thus the axioms of geometry are not concerned with space-relations only, but also at the same time with the mechanical deportment of solidest bodies in motion. The notion of rigid geometrical figure might indeed be conceived as transcendental in Kant's sense, namely, as formed independently of actual experience, which need not exactly correspond therewith, any more than natural bodies do ever in fact correspond exactly to the abstract notion we have obtained of them by induction. Taking the notion of rigidity thus as a mere ideal, a strict Kantian might certainly look upon the geometrical axioms as propositions given *à priori* by transcendental intuition which no experience could either confirm or refute, because it must first be decided by them whether any natural bodies can be considered as rigid. But then we should have to maintain that the axioms of geometry are not synthetic propositions, as Kant held them: they would merely define what qualities and deportment a body must have to be recognized as rigid.

But if to the geometrical axioms we add propositions relating to the mechanical properties of natural bodies, were it only the axiom of inertia or the single proposition that the mechanical and physical properties of bodies and their mutual reactions are, other circumstances remaining the same, independent of place, such a system of propositions has a real import which can be confirmed or refuted by experience, but just for the same reason can also be got by experience. The mechanical axiom just cited is in fact of the utmost importance for the whole system of our mechanical and physical conceptions. That rigid solids, as we call them, which are really nothing else than elastic solids of great resistance, retain the same form in every part of space if no external force affects them, is a single case falling under the general principle.

For the rest, I do not, of course, suppose that mankind first arrived at space-intuitions in agreement with the axioms of Euclid by any carefully executed systems of exact measurement. It was rather a succession of every-day experiences, especially the perception of the geometrical similarity of great and small bodies, only possible in flat space, that led to the rejection, as impossible, of every geometrical representation at variance with this fact. For this no knowledge of the necessary logical connection between the observed fact of geometrical similarity and the axioms was needed, but only an intuitive apprehension of the typical relations between lines, planes, angles, &c., obtained by numerous and attentive observations—an intuition of the kind the artist possesses of the objects he is to represent, and by means of which he decides surely and accurately whether a new combination which he tries will correspond or not to their nature. It is true that we have no word but *intuition* to mark this; but it is knowledge empirically gained by the aggregation and reinforcement of similar recurrent impressions in memory, and not a transcendental form given before experience. That other such empirical intuitions of fixed typical relations, when not clearly comprehended, have frequently enough been taken by metaphysicians for *à priori* principles, is a point on which I need not insist.

To sum up, the final outcome of the whole inquiry may be thus expressed:

(1.) The axioms of geometry, taken by themselves out of all connection with mechanical propositions, represented no

relations of real things. When thus isolated, if we regard them with Kant as forms of intuition transcendentally given, they constitute a form into which any empirical content whatever will fit, and which therefore does not in any way limit or determine beforehand the nature of the content. This is true, however, not only of Euclid's axioms, but also of the axioms of spherical and pseudospherical geometry.

(2.) As soon as certain principles of mechanics are conjoined with the axioms of geometry we obtain a system of propositions which has real import, and which can be verified or overturned by empirical observations, as from experience it can be inferred. If such a system were to be taken as a transcendental form of intuition and thought, there must be assumed a pre-established harmony between form and reality.

THE DEFECTS OF THE BLOCK SYSTEM.

From "The Engineer."

WHILE the inquiry into the cause of the Radstock accident is yet pending, it would be proper to comment on the evidence which has already been given. We shall be guilty of no breach of propriety, however, if we state that the catastrophe supplies another example of the failure of the block system to ensure the safety of railway passengers. The facts with which we have to do are quite independent of official evidence. Two trains ran into each other on a single line of rail. They ought not to have run into each other, and they would not have run into each other if the modification of the block system used on the line had worked efficiently. The Radstock accident supplies an addition to a long list of calamities which have resulted from the failure of the system of working trains by telegraph signals. The Abbots Ripton collision may be cited as another example of the break down of a much vaunted invention, and if it were necessary we could cite a whole host of collisions which have taken place on lines all over the kingdom in spite of the most elaborate telegraphic precautions. It is evident, indeed, that unless the block system receives substantial improvements it can no longer be looked upon as affording more than a very moderate protection for life and limb. It is quite indisputable that the method of working trains by a space instead of by a time interval, is in theory quite perfect. No device can be more ingenious, while the appliances which the use of the invention involves have been schemed and constructed with great care and skill, and have been adopted almost without regard to expense. But the fact remains that disastrous railway collisions take place with alarming frequency, and that the block system has not hitherto secured immunity from accident. We fail to see that any material reduction has been effected of late years in the number of those killed or seriously injured by collisions; and it will be found that on sections of road where the block system has not yet been introduced, railway travelling is at least as safe as it is anywhere else. The advocates of the block system argue, in reply to such a statement as that which we have just made, that although it is true that accidents do occur, yet that their number is materially diminished by this mode of conducting railway traffic; and that, in a word, without the block system there would be many more accidents than occur with it. This is obviously begging the question. It is impossible either to prove or to disprove such a proposition; but it is none the less true that the block system fails to prevent collisions. It is urged that the traffic on some lines—the Metropolitan for instance—could not be worked at all without block signals. This we admit; but the block system has entirely failed on more than one occasion to prevent very serious collisions on the Metropolitan and Metropolitan District railways.

Let it not be supposed that we are opposed to the block system; on the contrary, we believe that it is the most per-

fect possible method of working railway trains, in theory; but we cannot shut our eyes to the fact that its practice is altogether inconsistent with its theory. The system is, however, not only capable of being improved, but improvement is absolutely necessary before it can adequately supply the demands made upon its powers. Our purpose is to point out some of its most glaring defects, and to suggest a remedy. Under the old system of working, the passengers in trains had to rely almost altogether on the station-masters, drivers and guards for safety. Now, as a rule, these men represent the higher order of railway intellect, and the drivers and guards at all events, were prompted to vigilance by the knowledge that they carried their lives in their hands. The introduction of the block system at once transferred the responsibility of working the line to signalmen. With all respect for the class, and with a full knowledge that honorable exceptions exist, we must add that signalmen represent an order of railway intellect below that of station-masters, guards and drivers. They know, too, that a mistake will not endanger their own lives, and thus they lack a stimulus which the other classes possess. Again the duties of a driver are comparatively concentrated, while those of a signalman are extremely diffuse. To a driver, the train of which he is in charge is for the time being his universe. On it his whole attention is centered, to its management all his exertions are directed. He has nothing to think of but his engine and the load behind him, the road he traverses, and the signals for which he has to look out. But the case is totally different with a signalman. To him a train is but a unit —one of a hundred units with which he has to deal in the course of his day's work. His duties are harrassing to the mind and wearisome to the body. In one word, all the conditions are favorable to mistakes, while those under which a driver works are favorable to accuracy. But the block system has greatly simplified the work of the driver, the guard, and even of the station-master. Under it, the signalman is absolute lord of the situation. If he gives "line clear" not only does the driver go on his way, nothing doubting, but he is bound so to go. The dictum of the signalman is not to be questioned. Thus, then, the responsibility for the management of railway traffic has been practically transferred by the introduction of the block system from a higher to a lower order of railway intelligence; and we find a grave responsibility lifted from the shoulders of men earning £2 to £3 a week to those of men who earn a pound, or lads who receive 14s. or 15s. a week. It is impossible to read the evidence given at most of the inquests which have occurred under the block system, without becoming painfully aware that the right man was not in the signal boxes when the accident happened. In many cases poor souls do their best, while in others idle or incompetent men do their worst. The result, as far as concerns the railway passengers, is the same.

The remedy for all this is easily found, but we fear it will not commend itself to railway companies. Obviously, the men employed to work block signals should be of much better class than those now in the signal boxes. They should be grave, steady, intelligent men; not harum-scarum boys, overworked youths, or men of dense intellect, easily getting into a muddle, and liable to make mistakes. Strict sobriety should be insisted on; not that branch of sobriety which is represented by a man who will not drink on duty but will get drunk off duty as often as he has the chance, and who comes to his post a few hours after a debauch with his brain yet reeling. But men of the type required cannot be had for small wages. If it is worth while to pay a driver 7s 6d. a day, the same amount expended on a signalman should be equally laid out. The employment of really good men would go far to render the block system what it professes to be. Before we quit this branch of our subject we wish it to be understood that we have no desire to disparage signalmen as they are, but we do assert that they are not, as a class, quite what they ought to be; and the better men of the class will be the first to agree with us in this matter. It must be borne in mind that one incompetent man may render the competence of a large number of his fellows valueless. Nothing on earth is more vexatious to a really first-rate signalman than to be telegraphically united with a

dullard at the next station. A private code of swearing has been adopted on certain sections of most lines, and we can assure our readers that it is often vigorously employed. Another and obvious defect of the block system is that sufficient wire accommodation for the transmission of messages is not supplied. The wires are overcrowded, and important communications are delayed—as, for example, in the case of the Abbots Ripton accident. Some of the overcrowding, however, results from the use of the wires for "larking." When a man is left alone in a dull box, far away from his fellows, it is perhaps natural that he should use the telegraph to chat with his acquaintance at the next station, and there can be no doubt that important messages are sometimes retarded in this way.

The remedy for this, and one or two other abuses, lies in the use of printing instruments, so constructed that the messages delivered and received will be placed on record beyond the control of the signalman. If this were done it would be impossible to meet with an instance of A swearing that he had sent a message, while B swore that A did nothing of the kind. That the block system is capable of great things we are willing not only to admit but to maintain; that the block system, as it is now managed, is able to insure the safety of railway travelers, no one in the face of recent events can, we think, suppose; and a very serious responsibility devolves on railway companies who rest content with half measures, when the means of obtaining almost absolute immunity from collision are within their grasp.

ON THE CONSTRUCTION AND MAINTENANCE OF THE PUBLIC HIGHWAYS.

By Mr. E. B. ELLICE-CLARK, C. E.

From "The Builder."

The construction and maintenance of high-ways must form at all times a most important work of sanitary authorities, not only because of the increased comfort to the community of well-kept roads, but because of the increased cost of locomotion and cleansing and discomfort of badly-maintained roads. Unfortunately, the construction and maintenance of highways is a subject that few surveyors of the present day give much attention to. It is popularly supposed that the mere covering of a road with broken stone is sufficient to form a good and permanent highway; this being the view taken by a large number of those who have the control over roads, the consequence is that in many cases throughout the kingdom the roads are in a bad state of repair, and cost a larger amount annually than is necessary for cleansing and watering. Roads, too, are frequently maintained by committees, who have but a slight and imperfect knowledge of what their duties are; and I take it to be the duty of every surveyor of highways not only to advise his committee of the quantities of materials that are required to maintain these highways under their direction, but further to inform them of the best known means of maintaining the roads in a maximum efficiency at a minimum cost. Thus the surveyor secures the valuable aid of disinterested persons living at all parts of the district, in supervising the repairs, and indeed, without a large and expensive staff, with anything like a long mileage town roads cannot be kept as they should be unless the surveyor is thus assisted. I therefore propose in this report, as briefly as possible, to point out the cardinal points to be observed in the construction and maintenance of roads in Urban Districts such as Derby.

The direction of roads is usually determined by private capitalists, but it should, in my opinion, form the subject of legislative enactment, that all new roads should be laid out in certain directions, especially in rapidly increasing

towns; for were a preconcerted plan to be carried out piecemeal by owners of property, not only would a large amount of money be saved, but public convenience would be served in a much greater degree than it is at present, where every owner of property makes roads to suit the fancy of his surveyor or himself, acting frequently in ignorance, when he would only be too glad to follow out a plan which, in nine cases out of ten, would cost him no more money, become as available for his own purposes, and be of much greater convenience to the public at large. I am therefore strongly of opinion that where new roads are projected the sanitary authority should exercise their discretion not only as to the constructive portions of the roads, but also as to their direction; for we must not forget that all these new roads ultimately form part of that network of highways which is the only means of communication from one part of the district to another, and where roads are judiciously laid out at first, the stoppage of one street like South Street, by the Great Northern Railway, would be a matter of slight instead of great and permanent inconvenience.

The next thing to be considered is the gradients of roads. All roads should be as nearly level as possible. To illustrate this, I may cite the instance of Bridgegate. Suppose from its lowest point to the summit at King Street we had a perfectly level roadway instead of having a gradient of 1 in 32, as we have, every horse drawing a load would be able to draw one quarter more than it does now; and as we have 1,268 carts passing up this street daily, one half carrying 1½ ton each, 238 more tons could be taken to and from the town with the same amount of horse-hire each day. Then, again, roads with steep gradients cost fully one-third more to repair than flat roads. In the case of Bridge Gate it is three times more than London Road; consequently it is difficult to form an idea, without going into detail, of the increased cost to the community of steep roads, which should in all cases, where practicable, be avoided, and any extra expense in making urban roads level at the first outlay will fully repay the authorities by the ultimate diminished cost of cleansing and repairing, to say nothing of the saving to the community by the increased weight that can be drawn with the same amount of horse expenditure. One of the most important matters in forming roads is the cross section, *i. e.*, the shape from kerb to kerb. The prevailing profile now adopted is that of an elliptical projection, high in the center, and sloping gradually to the channels,—a section which is, in my opinion, the reverse of what it should be for roads sufficiently wide for four vehicles to pass abreast; and no main roads should be made a less width than this. My objections to the existing section for such roads as Friar Gate or London Road are, that if the road is made sufficiently flat for carriages to run easily over all parts of the roadway, the gradient is not sufficient to carry away the water which lies in the center, and renders the crown rotten. In suburban roads the traffic is nearly always in the center, consequently they are worn down most unevenly; this is manifestly a wrong result, as roads should be of such sections as no preference would be shown to any portion of their surfaces.

The prevailing section of roadways comes about by engineers following the footsteps of Telford, who constructed all his roads to the elliptical section; but the conditions existing in his time are most dissimilar to the conditions and exigencies of our day. Telford's roads were highways from city to city, having few vehicles running on them. The great objects then were not what they are now, to present over the entire area the hardest and least slippery surface, to give the firmest foothold to horses, and to stand the daily wear and tear of thousands of vehicles heavily laden, but to keep the roads dry and sufficiently hard in their centers to bear the traffic of a few vehicles, rarely exceeding a hundred daily; the elliptical section suited those times and requirements; vehicles rarely meeting, consequently the inconvenience arising from the steep sides of roads was not felt, nor was the center worn away, as the traffic was trifling. My own opinion is that all main roads should be made sufficiently wide for four vehicles to pass abreast, and where the traffic is considerable, and the rainfall is such as to necessitate pitched channels and storm-water sewers, the channels should

be in the center instead of at the sides, thus obviating the tendency of vehicles to slide down the sides towards the kerb; the slopes to the center channels from the kerbs need only be very slight, especially where the hard metal is used to coat the surface, in which cases a maximum of one in forty might be adopted. The advantages claimed for the section of road with channel in the center are:

1st. In the case of a Macadamized road with gullies and pitched channels, half the cost of both these serious items would be saved.

2d. The gullies need not be trapped, but be left open as sewer ventilators.

3d. Carriages would use all parts of the road surface except the channel, producing a uniform wear.

4th. The "slop" which is now swept to the sides of the road, thrown up to the carts, frequently bespattering pedestrians, would be swept to the channel in the center, and be the farthest possible distance attainable away from the foot-passengers and shops.

5th. In the case of a crowded thoroughfare, the channel would divide the up and down traffic without the aid of police.

The objections that have been raised to this section of roadway are:

1st. The carriages slide down towards each other, especially in frosty weather.

2d. A large stream of water washes away the metal at the channel.

My answers to these are, that the slope of the road to the center will only be slight, just sufficient to carry away the water, and not sufficient to cause the tendency of vehicles to slide to the center to have any foundation in practice.

The second objection is overcome in the case of sewered streets with frequent gullies, for the water disappears down these before it has assumed sufficient volume to become a stream having velocity enough to wash away the metal.

I am quite aware that this section of road-way is contrary to all the practices and prejudices of surveyors in this country; but I believe, if tried, we shall obtain better roads at less cost both in construction and maintenance, better lighting, and a more effectual mode of ventilating our sewers, all objects of paramount importance to Sanitary Authorities.

The next important matter in connection with roads is the nature of the materials of which they are formed and coated. The ordinary mode now in practice is to lay in a bottom of hard core, rammel, foundry cinder, or some such kind of material, to a depth varying from nine inches to one foot, without any uniformity as to size. The practice of the first road-makers in the world, viz., the Romans, and also the great road-makers at the commencement of the present century, was to lay courses of large-sized stones by hand on the road-bed to form a foundation, which, as a matter of fact, was the actual road, the next coating of broken stones being merely a covering to take the traffic wear, and prevent the road proper beneath from being worn into holes. The advantages of this method of forming a road are, that as it has to sustain great pressure from heavy loads and "violent percussion," it forms a solid foundation which is necessary to resist these. In the case where the tyres of the wheels are narrow and the loads heavy, the modern system of roadmaking is most pernicious. Under the old system, this kind of traffic is dealt with much more economically, as the weight is distributed over a much larger area, thus preventing clay and other soft material from "spewing" up when the road has become out of repair and soddened with wet. We have an excellent example (?) of this in the Mansfield Road, near Little Chester Church, where the surface has been raised very high from constant metalling on a bad bottom, but which no amount of metal short of re-making will form a good road, and where, in spite of frequent repair and scavenging, the road is never in good order.

Another great advantage of forming a rough pavement like the one I speak of is that it forms a drain for the water, which finds its way through the top coating of broken stone,—an especial advantage on a clayey soil like that of Derby. A road formed in this manner will cost but a little more than if formed by the prevailing mode, as stone of a much more inferior quality and less costly character may be used; it will bear a much greater amount of weight;

the bottom will not be influenced by the most severe frost and rapid thaws, and a less quantity of broken stone may be used than if the bottom is of small stone or hard core thrown down at random. In addition, the covering of broken granite or other hard stone will last much longer on a foundation of this sort, and the expense of repairing and scavenging will be considerably less. Sir John Macneil, from actual experience, computes the saving by forming a road like this at least one-third the expense of repairs annually, and I believe this to be within the mark.

I should like to say a few words on the use of small round stones (boulders) used on some outlying roads, and state my objections thereto. These small stones, unbroken, being pressed by vehicles against each other, find no "bed" when they come into contact. By such pressure they move, and thus form hills and depressions on the surface; and the joints being large, gaping in fact when compared with those of cubical stones, a large portion of the exposed surface joints becomes filled with horse-dung and mud, which must give an offensive effluvium, and be prejudicial to health. The illustration of Mc Adam to a Parliamentary Committee, when explaining the advantages of using cubical instead of spherical stones, is conclusive: he had two basins, one filled with marbles of broken stone, the other with spherical stones, and requested the members of the committee to thrust their clenched hands into each basin. They thus had demonstrated to them that they could plunge their hands to the bottom of the latter basin with ease, and the marbles "spewed" up, while the greater the pressure in the other basin, the tighter and more compact were the contents; and thus it is on a larger scale with roads. I therefore recommend you to discontinue the use of all unbroken stone, however small.

I have hitherto been speaking of suburban roads, and if I have digressed somewhat from the original intent of this report, it has been an attempt to show that a large saving in cost, and a greater efficiency in construction and maintenance, may be obtained in the future on those roads where the traffic is not so great as to require paving on the surface. Let me now deal with roads where the nature of the traffic is such as precludes the use of a broken stone surface. Premising that it is difficult to determine where Macadamising should end and pitching or paving commence,—where it is cheaper to cover a road with small unfixed broken stone or to pave the entire surface with fixed blocks, it would appear, at first glance, that that road which has the greatest number of vehicles passing over its surface should determine this, but I am of opinion that such is not the case. The mere enumeration of the vehicles passing a given point on a road will not enable us, without other considerations, to draw a hard and fast line where it is more economical to pave roads in preference to Macadamising them. We must also take into account (*a*) the class of traffic, subdividing this into heavily weighted, spring, narrow-tyred vehicles, and carriage traffic; (*b*) the gradient; (*c*) the nature of the bottom; (*d*) the width, and the height of the buildings. In three of these considerations (*a*, *b*, and *c*) the Wardwick has superior conditions to Bridge-Gate: we thus see why the former is a more economical road to maintain than the latter; therefore, every street must be treated on its own merits.

To ascertain whether it would be a saving to pave certain streets, I have endeavoured to obtain the cost of repairing and cleansing the same. I have left the watering out of the estimate, as I cannot even approximately obtain figures sufficiently reliable to enable me to produce them here.

The traction on level roads has been ascertained to be nearly as follows:

Roadway asphalted	1.0
Roadway paved, dry, and in good condition	2.0 to 1.5
Roadway paved, and in mediocre condition	2.5 to 2.0
Roadway paved, but covered with mud	2.7 to 2.0
Macadam in good condition and dry	3.3 to 2.5
Macadam wet	3.3
Macadam in mediocre condition	4.5
Macadam covered with dirt	5.5
Macadam with the stones loose	8.5 to 5.0

This is a low estimate. Some careful experiments give, taking the maximum load a horse can draw on a gravel road as a standard:

On a best broken stone road	3	times as much.
On a well-made pavement..	4½	"
On the best stone trackways	11	"
On railways.....	18	"

Here we see that there is a loss of more than 100 per cent. of motive power between Macadam in a mediocre condition and a roadway paved and in good condition; and assuming that 951 tons are daily carried along a certain road 300 days in the year, and the average friction is 1.20th of the weight, the cost of transporting this burden is £1,664 per annum. If we reduce the traction by one half, as above, we reduce the cost of transport on this road to £832; but, assuming this to be a theoretical supposition, and we take it at half, viz., £416, this would practically be the actual sum saved in horse-hire alone, by having the street properly paved.

We now come to the question of cost of cleansing, &c. Taking the number of square yards of its area at 3,000, in round numbers, we have ascertained that the cost of cleansing the street is £80, 10s. per annum. From careful observation it has been ascertained—that four loads of mud or slop are removed from Macadam per 1,000 yards of surface, to one load per 1,000 yards of granite-paved surface, which is reduced to one-third of a load on a surface paved with wood. I will, however, take the quantiy at one-half, as we purpose to use granite as the material for paving this street hereafter; this will save £45, 5s. for cleansing. This calculation does not include the carting of the slop to a shoot, which would bring the total up to £100, a year while Macadamised, and the saving in cleansing if paved to £50, a year. The saving in maintenance will be this: the original cost per yard, including concrete, will be 13s., the cost of maintenance I have estimated at 4d. per yard per annum, the life of the road I have estimated at twenty-five years. This gives us a total cost of a little over 10d. per yard per annum, or £125, for the whole, as against £216, 6s. 6d., the present cost, thus effecting a saving of £94, per annum; add this to the £416, and the foregoing £50, and we effect a saving of no less than £560, per annum to the community at large, to say nothing of the freedom from dust and the increased comfort to every person using and living in the road.

From careful observation I should say, all other things being equal, it is cheaper to pave a roadway having a traffic of one thousand vehicles per diem over its surface than to Macadamise it, and this would be the minimum number to commence paving with.

We must now inquire and specifically lay down what are the requirements for such paved streets. The engineer of Birmingham has admirably set them forth in a recent report to the Council of that borough. He says: "They should be even-faced, firm, hard, as noisless as possible, able to bear any burden, with sufficient foothold for horses, and capable of being easily cleansed so as to diminish as much as possible the annoyance arising from mud and dust; the nearer these objects are attained, the less will be the wear and tear of the roads as well as of the vehicles; property and health will be less injured, and the comfort of pedestrians increased."

These, then, are the desiderata of Highway Authorities, and, as will be seen at a glance, most difficult of attainment. To obtain noiselessness and the least friction the surface must be smooth; to obtain a firm foothold for horses the surface must have a certain amount of roughness (I use the word in place of a better one which I cannot find, though it hardly expresses my meaning), so that two of the desiderata are inverse to each other, for as we decrease the noise and friction we increase the slipperiness. To endeavour to make these meet the material should be as hard as possible, providing it gives the requisite amount of roughness of surface by attrition.

You are aware that during the past few years various materials have been introduced to supersede stone for paving streets having large traffic and heavy burdens; and though the experience gained by the engineers of London, Liverpool, Birmingham, and Bristol, and other large towns, is of much value, I think we may fairly say that the period of time over which their observation has extended, in reference to asphalts and wood, is insufficient to enable them to come to a final conclusion as to the merits of these respective materials. That this is so may be gathered from

the very opposite opinions given, nearly all based upon short-timed observation or theories. As to the durability, cost, noiselessness, and slipperiness of materials for roads, there should be no doubt whatever; but these can only be ascertained by actual observation, extending over a period of years, in streets with varying traffic as to number and nature of vehicles and weight of loads; and it would be unwise, most unwise, to reject a well-tried material for others, some of which have been pushed into the market without trial of any sort whatever. On the other hand, it would be unwise to expend large sums of money in laying stone pavements if there are other obtainable materials more suited to paving purposes. I shall therefore suggest that trials be made with certain materials. Previously, however, to my so doing, I will proceed to examine their qualities. For this purpose I have communicated with the engineers of most of the large towns, and have personally visited several. If durability and cheapness only had to be considered, so far as my observation goes, there can be no question as to the superiority of granite over all other materials used to pave streets with, more especially if laid so as to form a tram or trackway for the wheels of heavily-laden vehicles to run along. And this opinion is shared by all those who have lengthened experience of the requirements of busy streets; but these are only two of many considerations to be borne in mind. There are also safety to horses, noiselessness, easy traction, freedom from mud and dust. Hence the attempts to supersede the use of granite as a paving material. Probably the desirability of decreasing the noise, caused by the vibration of vehicles in business thoroughfares, has had more influence than anything else in these attempts to supersede the hard granites; and we must bear in mind that however desirable a perfectly noiseless pavement may be, in some thoroughfares it is a question that we can afford to discard, if purchased at too high a cost, viz. those streets, where neither the occupations of the inhabitants nor the pedestrian traffic is such as to force this desideratum forcibly upon us.

So far as I can ascertain, wood blocks, squared like granite pitching, appear to meet more of the requirements than any other of the newly-introduced materials, if we are prepared to put on one side the question of cost altogether; though, on inquiry, I find a host of objections urged against its use, some of which are worthy of attention;—the first being its permeability to wet, the separation of its fibres, and consequent absorption of "dung and putrescent matter," which are said to become "highly noxious." Again, it is urged that the blocks are separated by concussion, thus opening the joints and permitting the wet to reach the foundation; the wood thus being constantly wet, in hot and dry weather gives off, in evaporation, very foul matters, which taint the atmosphere sufficiently to injure health. These are allegations difficult of refutation; indeed, by actual observation in some of the London streets I am convinced the entire area of wood pavement is constantly saturated, and that it wears down very rapidly when compared with granite, under the narrow tyres of continuous omnibus and railway-van traffic. How far the objections as to tainting the atmosphere are valid I must leave those who are more competent to judge to decide, and confine myself to its actual cost, durability, and general suitability as a paving material.

In Bristol, one, or a portion of one, of the main streets was laid down, and eleven months after the borough engineer reported that: "The general appearance and surfaces of these pavements continue up to present time in much the same condition as when laid, but I have caused portions of the Wine Street paving to be removed at either end of the street, in order to ascertain its state, and can find no perceptible difference in the same at either opening, there being no appearance of decay. The wear of the surface of the blocks is scarcely perceptible; but at the Union Street end of Wine Street the surface-water has percolated through the joints of the wood to the foundation: this has rendered the pavement at this end of the street less rigid, and the constant working by the passing of the traffic over it has caused several of the blocks to split or crack, and it is probable will necessitate the relaying of this end of the street before long."

Two years' experience enabled Mr. Ashmead to confirm his opinion as to

the leakage of the joints destroying the pavement, for he says:—"Although nearly twelve months have now elapsed since my last report on this subject, the general appearance and surfaces of the pavements in Wine Street, Broad Street, and the Exchange, still continue in much the same condition; nor have the portions of Wine Street (relaid by the company) been much improved thereby, as the surface water finds its way through the joints, and a larger proportion of the blocks in this street are split or cracked: but the pavements of Broad Street and the Exchange appear to be wearing well, and are more rigid and firm, and free from the defects of the Wine Street pavement. This last paving has now been laid about twenty-three months, and Broad Street and the Exchange about seventeen months."

Writing personally to me, Mr. Ashmead says: "The wood will require repair in four or five years, but the comfort is so great that some people prefer paying for the luxury; but there is nothing like granite for durability and economy." How far we should be justified in laying down certain streets with a costly and luxurious article out of the general rates it is for the local authorities to determine.

The evidence of Mr. Deacon, C.E., the engineer of Liverpool, is to the effect that the pavement laid in Bold Street, in September, 1874, has worn but little.

The borough surveyor of Birmingham says that "wood presents an even surface, affords good foot-hold for horses, is pleasant for riding over, free from noise and mud,—dust, however, rising rapidly; it is capable of being easily cleansed, but, like all other pavements, it is somewhat slippery in certain states of the weather."

I have made observations on the condition of wood pavement in London; they are several, and I have extracted from a recent publication descriptions of them:

Croskey's Wood Pavement.—Several plans of wood paving have been of late years proposed. One by Mr. Croskey was to manufacture cross-grained planks of wood of any length, which, being placed side by side, are forced together by pressure, so as to form a compact homogeneous surface of wood, which was to be laid upon concrete. This plan does not seem to have been tried, at least in this country.

Harrison's Wood Pavement.—This system consists of a concrete foundation, upon which strips of wood, two inches by half an inch in thickness, are laid. Wood blocks three inches in breadth, are placed upon these wooden strips, and heated asphalt poured into the joints, which penetrates under and adheres firmly to the blocks.

Henson's Wood Pavement.—The main principle on which Mr. Henson proposes to construct his paving, is placing *felt* on the bed and between the joints, thus giving, as he asserts, an elasticity to the road, and allowing by the felt jointing for the expansion and contraction of the blocks. In one roadway they are levelled on the top edges; in another at every fourth or fifth block; and in another a V-groove is to be cut down the center of the block, the object of this last being to prevent the surface-water from soaking down the joints to the foundation.

The Ligno-Mineral Wood Pavement, which is laid in Grace Church, Fore, and Coleman Streets, possesses certain peculiarities, and differs from others in that it is mostly composed of hard wood, such as elm, oak, beech, or ash, which has been subjected by hydro-carburetted oils to a treatment termed mineralization. The blocks, which are nine inches long, three inches wide, and six inches deep, are sawn at an angle of about sixty degrees, the object of this being to expose the fibre obliquely to the wearing face, and to distribute the weight of the traffic from the one block to those adjacent to it in the line of thrust. On the sides of the blocks, near their bases, a groove is cut, which is filled with an asphaltic mastic (pitch and tar), during the process of laying. The blocks are laid on a concrete foundation, the angle of each course being in an opposite direction to the previous one. The remainder of the joints to the surface are filled in with a grouting of lime and gravel.

Carey's Wood Pavement is perhaps that which can be most rapidly laid down and repaired, and is formed as follows, viz.: the blocks are cut four inches wide by nine inches long, and to the depth of five inches or six inches according to traffic; these blocks are shaped with

alternate convex and concave ends, and are laid on a bed of ballast or sand, averaging two inches; the joints which have been left about $\frac{3}{8}$ inch wide, are filled with a grout of lime and sand. The advantages claimed for this pavement are, that by the peculiar shaping of the blocks they will not be shifted from their positions; and further, that the weight that may be put on each block is thus dispersed over an extended area.

Messrs. Mowlem & Co.'s method of laying wood paving is to form a foundation of concrete, varying in thickness according to the nature of the subsoil and the traffic; then to pave with blocks of yellow deal, three inches wide and six inches or seven inches deep; the joints which vary from $\frac{3}{8}$ inch to $\frac{1}{4}$ inch are filled in with sand and lias lime, and the surface is afterwards indurated by strewing it with shingle.

The Improved Wood Pavement is formed of two layers of one inch boards laid transversely and longitudinally on the old road foundation, which is made up with sand or dry earth to the proper curvature; on these boards wood blocks are placed, the longitudinal joints being kept three-quarters of an inch apart by a fillet (which is nailed to the flooring) and the heading joints butting. The joints from the fillet up to the surface of the road are filled in with fine ballast, run with a liquid tar, and caulked with a machine made for the purpose: the road is then strewn lightly with small gravel and is ready for use. The principal advantage claimed for this system is that the flooring of planks forms an elastic foundation, and tends to distribute the weight equally over the whole pavement, while the additional elasticity will lessen the wear of the blocks.

The Asphaltic Wood Pavement.—The mode of laying this system of wood paving is as follows, viz:—A concrete foundation, composed of blue lias lime and ballast in proportions of one part of lime to five or six parts of ballast, is laid to a thickness of six inches and to the correct curve of the road. Over this is laid a layer or bed of mastic asphalt half an inch in thickness, upon which wood blocks are placed in transverse courses with the grain of the wood upwards, a space of half an inch (or more if desired) being left between each course. Into the spaces, or joints as they are more usually called, heated asphalt is poured to a depth of from two inches to $2\frac{1}{2}$ inches up the block.

These are the pavements of wood, tried in London and elsewhere, and one cannot but be struck with the fact that only one attempt has been made in the metropolis to preserve the timber chemically, by such processess as Kyan's, Burnett's, Renwick's or Boucherie's.* In my inquiries I have only found another instance where any such attempt has been made, and that is at Sunderland. Here I find, from a report of the borough surveyor, dated 17th December, 1875, that as far back as October, 1867, a strip six feet wide was laid with creosoted Baltic red wood; a strip of three feet wide creosoted beech; and a strip of six feet wide with uncreosoted oak; and it appears that after eight years, with a daily traffic of 1,300 vehicles, the total cost of repair was £101, which was equal to a cost of $6\frac{1}{4}$d. per superficial yard. For the last five years, in those portions where the creosoted Baltic and beech, and uncreosoted oak were laid, no repairs have been necessary. The average vertical wear of the entire pavement has been $\frac{5}{8}$ of an inch, the Baltic has been $\frac{3}{8}$ of an inch, the beech $\frac{3}{8}$ of an inch, and the oak has not appreciably worn at all. The creosoted timbers are said to be harder than when first laid; the beech and oak are spoken of as being too hard and slippery. The surveyor says that the use of creosoted sets dispenses altogether with the cost of watering. This deduction is questionable, though the results are certainly the most favorable I have obtained, both as to wear and cost of wood pavements.

The method of laying a wood pavement by having longitudinal and transverse boards beneath for a foundation, is, to my mind, a faulty one. In the first place, by personal observation, I have seen a heavily weighted vehicle produce a depression of an inch as it passed along the pavement, which sprang up to its original form as the burden left it. Again, the "elasticity" which is claimed to be an advantage is, to my mind, the

* Kyan saturates the blocks with a solution of bichloride of mercury; Burnett with a solution of chloride of zinc to be absorbed in a vacuum; Renwick boils the blocks in coal tar; and Boucherie uses impure pyrolignite of iron.

reverse. If the pavement sinks beneath the load it has to carry, it is like having a surface of india rubber; with this depression an inclined plane of one in seven is formed with a wheel four feet in diameter, and one-seventh of the load must be raised this inch; and this is continually going on with such a bottom. It is next to an impossible thing for the numerous joints in the area (a diameter about eighteen inches under three tons) thus affected by each wheel to remain water-tight, be they made with asphalt or pitch, and the water must pass through them on to the planking below, which, sooner or later, must be rotted. These open joints in the end must wear the edge of the blocks round,—a fact I noticed in Birmingham, on close examination, which caused a most unpleasant vibration as a light springed vehicle crossed them.

I am certainly at a loss to understand this insistance of some surveyors for elasticity in a paved road, unless it be for the preservation of the horses' feet, which do not last so long on the "stones" as on a softer road. As for the mechanical effect of traction, it is totally opposed to all laws of mechanics for easy traction; and McAdam's theory, of the top stones rubbing against a concrete bottom, and thus wearing round, and being displaced, —from whence I imagine the present desire for elasticity comes, as the top pavement is a fixture,—is untenable. I think it might be laid down as a rule that the harder the bottom of a road is the better, whether for wood, asphalt, granite, or broken stone, and if you do not start with this initial you will never have a sound roadway. The difficulty to be overcome appears to be in keeping the wet from percolating through the joints, which are made wide so as to give a better foothold for horses. I think it is a questionable mode of laying pitchers, whether of wood or stone. I prefer having the joints closer than I have hitherto seen them for wood; first because there is less chance of the water finding its way to the foundation; secondly, these joints always wear away quicker than the blocks, leaving innumerable places for the lodgment of horse-dung and filth; thirdly, the traffic wearing the pitchers round. A demonstration of this can be found easily. A small joint, so long as it is a perceptible joint, is a preventive to slipping, and enables a horse to rise if it falls.

There now remains an examination of asphalt as a material for paved roads.

The most noted of these is that of the Val de Travers Company, which is described as limestone, containing about 12½ per cent. of bitumen, obtained exclusively from Neuchatel, Switzerland. It is claimed for it that neither "atmospheric heat nor frost affects it." In laying it for roads, it rests on a bed of concrete sufficiently thick to stand the traffic. The asphalt in its natural state, reduced to powder, is brought to the works hot, and compressed with heated irons and rollers uniformly over the entire area to be covered, leaving no surface seams or joints. It is laid about two inches to 2½ inches in thickness.

The next asphalt that has attracted attention is that termed Limmer. This, like the previous material, is laid on concrete. It is brought from Hanover; mixed with a certain proportion of sand and bitumen (the company say that the asphalt contains from seventeen to twenty per cent. of bitumen); heated in caldrons and laid on to the concrete about two inches in thickness, and then is smoothed with irons to the requisite surface.

There are several other asphalts that have been tried, but only to a limited extent, except, perhaps, Barnett's which appears to be a mixture of pulverised iron ore and mineral tar made liquid by heat, and spread, as the other asphalts, about two inches thick.

The battle of the materials is almost as fierce as the "battle of the gauges." The promoters of asphalt and wood pavements both claim the same merits, and there appears to be a diversity of opinion amongst those competent to judge, as to which is the better material of the two. The engineer of Bristol says,—"My experience of asphalt is not large nor favorable: the first cost of Val de Travers was 16s. 6d. per yard, but it has not lasted here more than six years, and is now in a very bad condition; the wood will require repair in four or five years."

No asphalt is to be laid in the Birmingham roadways, as the gradients are so unsuitable.

As a material for footpaths, Mr. Till,

C.E., the borough engineer, says,—"I cannot speak favorably of its durability."

The City of London engineer, Mr. W. Haywood, C.E., has prepared an elaborate report on the subject, setting forth in detail the result of his experience of both wood and asphalt; and as no one has either had such opportunities or facilities to come to fair conclusions in the matter, I extract these conclusions *verbatim* from his report. They are:

"1. As regards convenience.—That asphalt is the smoothest, driest, cleanest, most pleasing to the eye, and most agreeable pavement for general purposes; but wood the most quiet.

2. As regards cleansing.—That wood may be kept cleaner than it hitherto has been, but will be more difficult and expensive to cleanse effectually than asphalt. That as both pavements require occasionally strewing with either sand or gravel, there is not much difference between them in that respect.

3. As regards construction and repair.—That asphalt and wood, taking all seasons and all weathers into account, can be laid and repaired with about equal facility; but that the smallest, neatest, cleanest, and most durable repairs can be made in asphalt.

4. As regards safety.—That weather considered in reference to the distance which a horse may travel before it meets with an accident, or the nature of the accident, or the facility with which a horse can recover its footing, or the speed at which it is safe to travel, or the gradient at which the material can be laid, wood is superior to asphalt.

5. As regards durability and cost.—That wood pavements, with repairs, have in this City had a life varying from six to nineteen years, and that with repairs an average life of about ten years may be obtained; that the durability of the asphalts is not known, but that under the system of maintenance adopted, they may last as long as wood; that contrasting the tenders for laying and maintaining for a term of years the two best pavements of their kind, wood will be the dearest."

We must remember that Mr. Haywood is speaking of ordinary fir timber unpreserved in any way. Probably his opinion would be materially altered if he was aware of the durability of the creosoted timber in Sunderland.

Mr. Haywood, in reporting on the comparative safety of these materials, says that a horse would travel 132 miles before a fall took place on granite; 191 miles on asphalt; and 446 on wood. This is the experience of London streets; but we cannot be guided by that in Derby. It is the practice in London to move all railway vans, and a large proportion of heavily-weighted vehicles, at a trot. With us such vehicles are transported at a walking-pace, and as accidents increase *pro rata* with the speed and load, we should not be liable to anything like so many.

My own comparisons of these materials are: that for a very large number of streets where the roadway is fairly wide, where there are few shops, and where the occupation of the inhabitants is such as noise would not be a great objection, granite is the cheapest and best material that can be used. That in streets of business, where absence of noise is a desideratum, preserved wood paving is the best, though it is expensive. That asphalt is an impracticable material to use in our town, where it would frequently have a greasy surface, unless there is some means of making it less slippery in damp weather. I have had submitted to me a model by the Val de Travers Co., which shows an introduction of small iron studs on the surface, with asphalt between; but my experience goes to prove that the mixing of two materials differing so essentially in their toughness and hardness is quite wrong; that the studs would in a few years be left so high by the wear of the asphalt as to be dangerous, and I cannot recommend you to lay any large area with this, though the Val de Travers Co., of Birmingham, are willing to lay a trial piece and charge nothing if it is not successful, and it would only be fair to give them a trial.

THE Baldwin Locomotive Works have just completed the first locomotive for street cars made by them, in which the boiler and machinery are separate from the car. A trial trip will soon be made on the Market Street Railway, Philadelphia.

ON WELDING IRON.*

By Mr. RICHARD HOWSON.

From "Engineering."

There are, doubtless, some here present to whom the views contained in the following remarks are familiar, and, perhaps, some who may consider them self-evident and superfluous. Nevertheless I believe them to be not uncalled for, inasmuch as their object is to combat a misapprehension commonly current at the present time respecting the nature of puddled iron. This misapprehension is, that a bar of plate, made out of several pieces welded together, is not so good as one made out of a bloom formed from a single puddled ball.

It is said that an impending change in the manufacture of finished iron is inevitable—machinery must take the place of handwork. There are sound reasons for believing that this opinion is likely to be verified, but along with these there are other unsound ones which lead to false conclusions.

We frequently hear the remark that in order to compete with steel, we must have homogeneous iron, meaning thereby iron rolled from a bloom made out of a single puddled ball—that iron must be puddled in large masses, that it must be balled up in one mass, and not divided in the furnace into separate balls, that the larger the ball the better—15 cwt., 20 cwt., and even 30 cwt. being thought not extravagant, that the system of piling must be abolished, that piling means welding, and that welding is a mistake. Now, my object is to point out, in the first place, that iron rolled from a single puddled bar is not homogeneous; 2d, that, within certain limits, the larger the mass the more heterogeneous the iron is likely to be; and 3d, that the system of welding is not so bad as is supposed, but, on the contrary, has its advantages, if properly carried out.

By way of parenthesis, I may here state that the term "homogeneous" can only be strictly applied to iron which has been melted, or mild cast steel containing a low percentage of carbon. Mr. Howell, of Sheffield, who was one of the earliest workers in this field, has furnished me with a sample of such metal, and Messrs. Vickers, Sons, and Co, with another. They are both beautiful materials, nearly as low in carbon as ordinary wrought iron. Both are ductile in an extreme degree, capable of even being rivetted cold. In tensional strength, they are not so high as steel, varying from twenty-seven to thirty-six tons per square inch, according to the amount of carbon they contain.

One of these specimens has been hammered, and the other rolled, and one face of both of them has been polished and treated with acid, in order to exhibit the structure. When compared with the face of ordinary puddled iron similarly treated, the distinction is at once seen. They show no fibre or lamination. On the other hand, here is a piece of Cleveland bar of good quality. The fibres stand out like whip-cord; and here is a piece of Low Moor cable iron, which, to a minor extent, presents the same ribbed condition. They are in fact, anything but homogeneous. It must not, however, be imagined that they are weak in proportion to the damaged appearance which the acid has imparted to them. The tensile strength of the Cleveland bar is twenty-seven tons per square inch, while the Low Moor is little, if any, inferior to the average of the two homogeneous samples.

To return to the subject. I have here a piece of ordinary puddled ball. It has been cut in two, and the section polished. Its entire mass consists of a number of nuclei of tolerably pure wrought iron, interlaced with cinder, or with cavities which have once been filled with cinder in a liquid state. Such a ball, when compressed under the hammer or squeezer, becomes, by the uniting together of its nuclei, a more or less solid mass of iron. The greater portion of the cinder is expelled, permitting metallic contact at a greater number of points and over a larger area.

We thus see that the term "homogeneous" is not applicable to a bloom

* Paper read at the Leeds Meeting of the Iron and Steel Institute.

obtained in this way. It is as essentially a process of welding, as is that of a mass of hammered scrap iron.

Such being the case, we cannot fail to conclude that the more perfectly the cinder is expelled the nearer we shall approach to homogeneity, and we have to consider what is the best way of getting rid of it. Of course, machinery and skilful manipulation must always play an important part in the operation; but leaving these out of the question for the present, it is of no less consequence to ascertain how far the influence of mass or size affects the desired end.

It is evident that whichever of the known systems we adopt, either the slow squeezer or the rapid blow, the larger the mass the less chance there is of obtaining throughout a uniform structure. There will always, under all conditions, be more cinder towards the inside of the bloom than towards the outside, and the larger the bloom, the greater will be the inequality. A long bar rolled from a single bloom must inevitably be more heterogeneous in structure—more subject to flaws, when it is large than when it is of smaller dimensions. This is the rational view of the case, and I confidently appeal for its confirmation to the experience of those whose efforts have been directed to the production of a high class of manufacture. It is certain that machine puddling helps towards the attainment of success in dealing with large masses, but good puddling is a distinct operation which stands on its own merits. I have shown that in all cases the process is one of welding, and if freedom from flaws is desired, it is a mistake to suppose that the employment of puddled bars of gigantic size tends to that object.

Let us now inquire what welding means, and what are the conditions for securing efficiency and soundness. There is the more necessity for this, as a somewhat common misconception exists as to its real nature. It has even been spoken of in this Institute with some degree of contempt, as a mere glueing together of the particles of iron with a film of cinder. It can easily be shown that such a view is founded on an entire misapprehension of the process.

When we bend a piece of iron, or stretch it, or twist it, or compress it, we experience a certain amount of resistance to motion. It requires a considerable force to compel the molecules to change their relative position within the limit of elasticity, and still more to change it permanently.

The attraction of the iron for itself holds the atoms a fixed distance apart, which is unalterable except by the exertion of a force greater than itself. If, on the other hand, the iron were rendered gaseous, the atoms would become perfectly mobile and self-repellent. When it is in a liquid state, the condition is neutral, and when it is viscous the attraction, to a certain extent, predominates. It is this attraction which effects the welding, and during the process it will inevitably expel any intervening liquid substance, which is less viscous than itself, provided the liquid has a chance of escaping.

If two globules of mercury, immersed in water, or even a more dense fluid, are made to approach each other, they will readily unite on a slight concussion. So soon as the metal comes within atomic distance, the fluid is expelled, and the weld is perfect. If the globules are covered with a film of oxide or dust, it becomes more difficult to obtain metallic contact, and the globules rebound instead of uniting. This leads us to a fair conception of what welding means.

It is the common boast of a good smith that he can unite two pieces of iron so firmly as to give way in any other part rather than the joint. Here is a sample of a bar which has been welded in the middle. Under a strain of nineteen tons it has given way, but not at the weld, and we may be sure that this could never be the case if welding were only a process of glueing. In reality, the splice ought to be the strongest part of the bar, because, in addition to joining, the process is also one of doubling and re-drawing.

As regards the means of obtaining complete metallic contact, the skill of the workman has to be exercised,—1st, in heating the iron sufficiently; 2d, in protecting the surface from oxidation by means of a flux; 3d, in forming the surfaces in such a way that the flux has a means of escape when the ends are closed up under the hammer. The natural flux is, of course, the oxide of iron

which forms during the process of heating; but this of itself is of an infusible character, and a welding heat not being a melting heat, it is liable to resist union by its very dryness. The workman, therefore, assists the fusibility of the oxide by the addition of a silicious sand. The office of silica in the operation is thus twofold; it unites with the oxide to form a glass which is fusible at a comparatively low temperature. The glass overflows the heated part and protects it from further oxydation, while its fluidity enables it to be more easily expelled when the union takes place.

It is often remarked that silica is the great enemy of iron, and when present in excess this is certainly the case, because then its own infusibility comes into play. When used with judgment and in the proper proportions, it is indispensable as a means of obtaining the requisite condition of fluidity. There are certainly better fluxes than silica, as for instance borax, and a mixture of silica and alumina is better than silica alone. The flux commonly used in Sheffield for welding cast steel is a dried brick clay, a sample of which, as analysed by Mr. N. Samuelson, contains:

	Per cent.
Silica	58.50
Alumina	32.95
Peroxide of iron	4.65
Lime	3.25
Magnesia	0.54
	99.89

The essentials of a good flux are in all cases a readiness to combine with the oxide of iron, and fusibility at comparatively low temperatures when so combined. We thus see that although much silicon in pig iron is undesirable in the puddling process, a certain quantity is advantageous, and even necessary, while it plays a most important part in the subsequent processes. Even in the thinnest film it performs its office, and when iron is worked over and over again, it is still present, constantly diluting and constantly permitting closer and closer metallic contact.

When we trace the action of this basic silicate in some operations, we meet with results which are really extraordinary. I have here a small piece of a large armor plate, kindly furnished by Mr. Ellis, of the Atlas Works, Sheffield. The plate, when finished, measured 14 feet 16 inches × 5 feet 4 inches × 2 feet and weighed 31 tons. In order to show the amount of welding which took place in the manufacture of this plate, let me observe that the whole is made out of puddled balls weighing a little over 1 cwt. each, while every puddled ball consists of thousands of metallic globules originally not larger than peas. The puddled bars made from these are cut, piled, and re-rolled; and the process of piling and re-rolling is repeated up to six times. We thus arrive at the somewhat astonishing fact that the entire mass consists of not less than 11,448 distinct laminæ, and that each inch contains 528 such laminæ. In the sample before us, the cross section has been subjected to the action of dilute hydrochloric acid, in order to exhibit the grain. It will be seen that the structure, although it cannot be called absolutely homogeneous, is little inferior to really homogeneous or melted iron. This is, of course, owing in a great measure to the extreme extenuation of the small quantity of cinder which remains in the mass after welding. Consisting, as the plate does, of six successive operations of piling and rolling, the last weld will enclose most cinder, and the first least for equal areas. What the enormous area is, over which the cinder of the first pile is spread, I leave those skilled in figures to calculate, and to draw their own conclusions. It is sufficient to say, that the plate altogether forms a remarkable proof, or rather upwards of 11,000 proofs, of the efficacy of welding.

I will now draw attention to the system adopted at Low Moor, which is also a system of welding, although essentially different from the foregoing. Quality being the great object aimed at, regardless of cost, the most minute care and precaution is taken at every stage of manufacture. The process in forming a plate is as follows: The pig iron is smelted with cold blast, from materials comparatively free from both sulphur and phosphorus, and is afterwards refined. It is then puddled in heats of not more than 2½ cwts., with the addition of nearly ½ cwt. of scrap, consisting of finished plate shearings. The puddling process ended, the balls are hammered, each into a separate slab. These

are then reheated, and doubled and reheated and doubled again, until the requisite weight has been attained, and the whole is then reduced by hammering (involving fresh reheating) to a slab of the proper dimensions and shape for the rolls. The rolling presents nothing unusual, except in one important respect. The slab is not rolled with the original laminations horizontal, but vertical or cross-grained. This is a precautionary measure for detecting flaws. If there is a blister or defective welding anywhere concealed in the mass, it is more likely to appear at the surface of the finished plate by this method than by the ordinary plan of rolling with the laminations parallel to the rolls. Nothing could afford a more striking proof of the reliable nature of a sound weld than this mode of manipulation, producing, as it does, work of such excellence. If the plate once passes inspection, no trace of the lines of junction is seen.

In the manufacture of bars even greater precautions are taken than in that of plates. Every slab from the puddled ball is, in this case, broken in two and the fracture examined. Every piece which seems to have been at all burnt, or which presents a shelly appearance, is rejected and laid aside for common purposes. Those which pass muster are made into piles, which are hammered and doubled as described above, previous to rolling, while, for best cable iron, the blooms are not rolled into the finished section direct, but into flat bars, which are again cut up, piled and re-rolled.

The reason for these extra precautions is this. In plates (which, being for boiler purposes, are not usually very thick), any flaw is almost sure to make its appearance at the surface; but in bars, especially of large diameter, it is possible for a defect to exist in a center, where it would escape detection.

I need scarcely add that the conditions of a sound weld are nowhere more appreciated than at these works, and the greatest care is exercised at every stage, even to the extent of employing an inspector, whose duty it is to examine every slab and remove every speck of dirt or shell which may appear on its surface, before it is put into the furnace.

I will now shortly recapitulate the points to which I have endeavored to call the attention of the meeting:

1. When two surfaces of wrought iron in a half melted or viscous state, are pressed together, the intervening cinder is expelled, provided it is liquid enough, and has a chance of escape, and cohesion results. This constitutes a weld, and if the proper conditions have been fulfilled, such a weld will be as sound as any other part of the bar.

2. The requisite conditions are seldom, if ever, absolutely fulfilled in practice. When the surfaces to be united have a large area, it is impossible to avoid enclosing some small quantity of cinder. The endeavor should be to reduce it to a minimum, and then dilute it by after working.

3. Shingling a puddled ball is essentially a process of welding, and the term homogeneous is altogether inapplicable to it.

4. The conditions for perfect welding are in no case favorable in a puddled ball, and the difficulty increases with its size.

I ventured, at the early part of this paper, to express some confidence that the views which I have endeavored to put into a practical shape would be assented to by many. I am fully sensible, however, that there are some who have both thought and worked on the subject, who hold adverse opinions as to the main point in question. They say justly: "The time has come when not only better iron has become a necessity, but also cheaper, and there is no way of economizing so promising as that of dealing with the material in large masses." They may perhaps fairly add that this has already been done with a fair amount of success.

Now, I admit the necessity of economy, and I do not deny that in machine puddling there may be some advantage in working on a large scale. This advantage, however, is not paramount, and has its own difficulties, besides which it imports greater difficulties into the subsequent processes. There is no shingling machine yet constructed which is at all competent to do proper justice to a puddled ball of one ton weight and upwards. If there has been the appearance of success in the trials hitherto made, it is only just to take note of the

failures, and it must be remembered that it requires many successes to pay for one failure.

What I wish to point out is, that the system is not likely to yield uniformly good results, while it involves the use of machinery of an enormously expensive character; that there is no need of a revolution so sweeping as to prove disastrous to old works, but that the machinery at present in existence, with very slight additions, is quite competent to deal with machine puddled iron with comparative economy, and with assured certainty as regards regularity of working. To take a single instance, let us suppose that the manufacture is that of rails.

I have shown that the oftener the iron is doubled and drawn, the more is the small quantity of cinder contained in it extenuated, and the tougher and stronger is the product. There is no doubt, however, that it is quite possible to make an excellent rail out of a single bloom direct, provided it is machine puddled and is not too large. The system, therefore, resolves itself into a series of puddling machines, each turning out from five cwt. to six cwt. at a heat, a small inexpensive squeezer to bring the ball into shape, and the present hammers to finish the bloom. This might all be effected at one heat, and another heat would finish the rail. With respect to the puddled bar train, that would come into force for finer qualities of iron by rolling down the blooms into bars for piling and re-rolling.

Supposing now, that instead of 6 cwt., we have to deal with 18 cwt. Such a ball may be supposed either to pass through all the processes in one piece until it produces a length equal to three finished rails, or it may be cut into three pieces after the operation of squeezing. In the first case, we have entirely to discard existing machines and substitute others of a more costly description, while the mode of working, if the foregoing remarks are to be at all relied on, involves comparative risk and uncertainty as regards quality.

In the second case, a squeezer of equally powerful dimensions is required to reduce the bloom into a shape ready for cutting, and no advantage is afterwards gained over the plan of commencing with smaller balls.

I need not refer particularly to other clssses of manufacture, the systems of which indeed are capable of considerable variation. The same general views, however, hold good in every case. An armor-plate, of however small dimensions, cannot be made out of one ball, because the first operation of squeezing folds up the cinder, which cannot afterwards be got rid of, except by a process which is not contemplated, namely, repeated drawing and doubling. So with all classes of iron. The supposed homogeneity of the single ball is, as yet, a mere conjecture, and likely to prove a fallacious one; while the economy of the system is more apparent than real. Granted that machine-puddling will before long entirely supersede hand labor, the question lies between working on a large scale and on a comparatively small one. If we sum up the advantages which may be claimed for the former, they appear to consist as follows: A small saving of fuel in some cases, a small saving in labor, a small saving in crop ends, and perhaps on the average, a small saving of waste in re-heating.

On the other hand, we have the sacrifice of existing machinery, and the expense of erecting new, of a very costly class. We have also the increased liability to loss through the folding up of raw iron and fettling in the puddled ball, and generally greater difficulty in obtaining a uniform and reliable product, accompanied with more trouble from wear and tear, and the alarming risk of long stoppages from the breaking down of gigantic machinery.

I venture to say that a year's experience of the two modes of working would prove in favor of the smaller furnaces.

I will now conclude this paper by expressing a hope that if the views which I have brought forward run counter to the experience, or to the matured opinions, of members present, they will give us the benefit of their knowledge. We have fairly arrived at an important epoch in the iron trade—a period of transition—but it is not all daylight before us, and the *festina lente* policy is the safest. While charging others with holding mistaken opinions, I may be in error myself. My only object is to contribute some little towards the solution of a yet uncertain problem.

AN ALGEBRAIC PROCESS FOR DETERMINING THE MAXIMA AND MINIMA OF QUADRATIC EQUATIONS.*

By SAMUEL H. SHREVE, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

A QUADRATIC equation, or an equation of the second degree, is the equation of a curve; thus, the equation,

$$H=\frac{w\,x}{2\,d}-\frac{w\,x^2}{2\,d\,l}$$

giving the strains in the chords of a horizontal truss, (w = the full uniform load, l = the length, and d = the depth of the truss, and x = the distance of the point at which the strain is sought from the abutment) may be represented by Fig. 1.

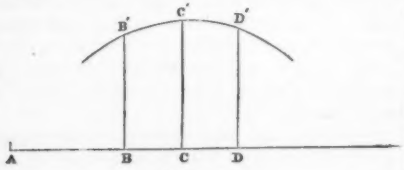


FIG. 1.

Here let A B, A C, and A D represent certain values of x laid down to a scale, and B'B, C'C and D'D the corresponding values of H, then the points B', C' and D' will be in a curve.

In some equations the curve will be convex to the line on which x is measured, as in Fig. 2.

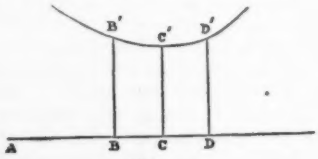


FIG. 2.

It is evident that, in such an equation, there is a certain value of x, which gives H, in Fig. 1, its maximum, and in Fig. 2 its minimum value.

This value may be found by the tedious process of substituting successive values of x in the equation, or, as is generally done, by the rules of Differential Calculus; but it is possible to determine it by the much simpler and more intelligible processes of Algebra, as follows:

Since the curve is uniform on either side of the maximum or minimum point C', when B C = C D, B'B must = D'D. Let H' = strain, at B, H that at C, and H'' that at D, Fig. 1, then to find the maximum value of the above equation,

$$H'=H'', \text{ or, } \frac{w\,x'}{2\,d}-\frac{w\,x'^2}{2\,d\,l}=\frac{w\,x''}{2\,d}-\frac{w\,x''^2}{2\,d\,l}$$

$$\therefore x'-\frac{x'^2}{l}=x''-\frac{x''^2}{l}$$

$$\text{and } x'-x''=\frac{x'^2-x''^2}{l} \quad \therefore l=x'+x''$$

Then since $x'+x''=2\,x$, $l=2\,x$, and $x=\frac{l}{2}$, is the value of x which gives the equation its maximum value.

The same reasoning, but not the same equation, will apply to Fig. 2.

A simple rule may be obtained by taking an equation containing a greater number of terms, such as,

$$H=\frac{w\,u\,x}{d\,s}-\frac{w\,x^2}{2\,d\,s}-\frac{3\,w\,u^2x}{2\,d\,s^2}+\frac{w\,u^2x^2}{d\,s^2} \quad (1)$$

which is the horizontal component of the strain in the unloaded part of the lower rib of a parabolic arch, the upper rib hinged at the center and at the abutments, the load covering more than half the arch, u = length of the unloaded part, w = the weight of a full load of equal density with the partial load, x = distance from the unloaded abutment of the point at which the strain is taken, s = span, and d = depth of the arch. It is required to find that value of u which makes the equation a maximum at the point x. Here u is the variable quantity, and proceeding as before,

$$\frac{w\,u'x}{d\,s}-\frac{w\,x^2}{2\,ds}-\frac{3\,w\,u'^2x}{2\,d\,s^2}+\frac{w\,u'^2x^2}{d\,s^2}=\frac{w\,u''x}{d\,s}$$
$$-\frac{w\,x^2}{2\,ds}-\frac{3\,w\,u''^2x^2}{2\,d\,s^2}+\frac{w\,u''^2x^2}{d\,s^2}.$$

* The substance of this article is from the Introductory Chapter to a forthcoming work on "Arch and Suspension russes," and is believed to be new.

$$\therefore\ u' - \frac{3u'^2}{2s} + \frac{u'^2x}{s^2} = u'' - \frac{3u''^2}{2s} + \frac{u''^2x}{s^2}. \qquad (2)$$

Hence we see that, at the first step, all terms which do not contain the variable and all common factors, disappear.

From equation (2), we get

$$u'-u'' = \frac{3u'^2}{2s} - \frac{u'^2x}{s^2} - \frac{3u''^2}{2s} + \frac{u''^2x}{s^2}$$

$$= \frac{u'^2}{2s^2}(3s-2x) - \frac{u''^2}{2s^2}(3s-2x)$$

$$= \frac{u'^2-u''^2}{2s^2}(3s-2x) = \frac{(u'-u'')\ (u'+u'')}{2s^2}(3s-2x),$$

$$\therefore\ 2s^2 = (u'+u'')\ (3s-2x).$$

And since $u'+u''=2u$

$$2s^2 = 2u\ (3s-2x) \text{ and } u = \frac{s^2}{3s-2x},$$

which value, substituted in eq. (1), gives it its maximum value.

It will be noticed that the last operation is equivalent, after having eliminated from eq. (1) the constant term and common factors, to dividing it by the variable, and multiplying the terms then containing the variable by 2. From this the following rule may be deduced:

To find the maximum or minimum value of a quadratic equation containing the first or second powers of the variable:

Eliminate all constant terms and common factors.

Divide by the variable.

Multiply the terms which now contain the variable by 2.

Make the equation so changed equal to zero, and determine the value of the variable.

This value of the variable, substituted in the original equation, will make it a maximum or minimum, as it is of the form $x-ax^2$, or ax^2-x.

If the curve were not uniform on either side of the maximum or minimum point, the above rule would still apply; for a curve may be considered to be composed of infinitely small straight lines, and the maximum or minimum point will be at the center of that straight line which is horizontal, and x' and x'' are measured to its ends. Hence the above reasoning will hold good in such a case.

Cubic equations have both a maximum and a minimum point, and a very slight elaboration of the above process will give a simple rule for determining them.

ELECTRICITY IN THEORY AND PRACTICE.

From "The Engineer."

At the present time, when telegraph lines and cables are receiving such rapid extensions, and when such a vast labor field is being opened up to both men and women, we may be allowed to call close attention to a very important subject. Reduction in the cost of messages has largely increased the number of messages, and experience thus tells us that further reduction means greater increase, and the increase of facilities for intercommunication means increase of business. There exists two ways by which the cost of messages can be reduced—one is the reduction of wages, which is not likely to occur, in fact the opposite—namely, a general raising of wages is more probable; secondly, a decrease in the cost of materials or increased efficiency in their use. The latter seems to be the only way in which reduction can take place. Iron, copper, wood, acid, &c., are not likely to be any cheaper, and thus we are reduced to the more efficient use of these materials. He who makes two blades of grass grow where one grew before confers a benefit upon the whole community; he who doubles the efficiency of a battery or a cable also confers a benefit on humanity. To do this we must look to science, to an education of workmen which will enable them to understand the powers and capabilities of the materials placed in their hands. Hitherto all that has been thought necessary is the supervision of a large number of rule-of-thumb men by a few who have a fairly practical know-

ledge of their work, combined with a modicum of theory. In our opinion theory should go hand-in-hand with practice, the one being incapable of advancing far towards perfecting the science without the other. As an example of the combination of practice and theory, we may point to Sir W. Thomson, whose work is the admiration of every electrician. As an example of practice without theory, Mr. Brough, in speaking of the use of Wheatstone's bridge, says: "Bridges for testing telegraph lines are almost invariably wrongly arranged." This is corroborated by Mr. Heaviside, who says: "As Mr. Brough most truly observes, most Wheatstone's bridges are wrongly arranged." Now, if men are unable to test their lines in the most efficient manner, can we expect their other work to be any better performed? Now that the telegraph lines are in the hands of the Government, the public can, by judicious criticism, insist upon efficiency. That a special education is required to make a man fit to undertake electrical duties is already felt, and means more or less good have been taken to supply the want. A few schools, public and private, make this subject a specialty. Little, however, can be said in their praise. Charlatanism and empiricism reign supreme here, as with the majority of cases of those who undertake to educate the middle classes. The science department pays some attention to the subject, but the practical results arising from these sources are unworthy of consideration. Ultimately, as our teachers get taught, some good may arise. Lastly, we have numerous text-books which are supposed faithfully to put forward the elementary facts of the science. Unfortunately, in very many cases these books are wholly unreliable, and in others a strange want of method, tact, and style detracts from their use.

It is absolutely necessary that a science should be taught scientifically, that looseness of statement should be avoided, and that a rigid adherence should be given to facts. Scientific writers in every other branch of science recognize this and act accordingly, but in electricity old terms long exploded are used over and over again, dummies made to do the duty of live men. Since the publication of Faraday's "Experimental Researches" there has been a lull in experimental progress. Strange it is that publishers, so energetic to issue worthless books, do not come forward with a reissue of Faraday's volumes. They are invaluable to the student, they are not out of date, and they can be obtained only with great difficulty—three very good reasons, we should imagine, for a new edition. Leaving out of consideration such works as Thomson's "Collected Papers," and Clerk Maxwell's elaborate "Mathematical Treatise," books not suited for the class which we have now under consideration, we come to a number of elementary books which might be thought useful. There exists no one work entirely suited to the wants of practical men. We do not mean to say no new books are published. A heap of these text-books lies before us, but our hearts are hardened and we refuse to accept them as worthy of the place they desire to fill. In writing a treatise on this subject, no matter whether intended to be exhaustive or not, a great difficulty has to be encountered at the outset, inasmuch as no definition of electricity can be given. It is at present almost entirely the science of phenomena. Its laws are deduced from observation. True, several theories are given as to what it is. Thus, Mr. Sprague says: "Only recently, and as a consequence of the complete conversion of the scientific world to the opinion that heat is a 'mode of motion' has electricity begun to take what is evidently its true place, as one of the phases of force or forms of motion, and yet only by thus regarding it can we possibly obtain a clear conception of nature and a perfectly satisfactory explanation of nature's phenomena." In other words, Mr. Sprague adopts the theory that electricity is a form of energy. On this point we have a fair field for discussion, and are not surprised to find some of the acknowledged masters of the subject directly and flatly contradicting the theory. Clerk Maxwell, after having given one reason against this theory, says: "There is, however, another reason which warrants us in asserting that electricity, as a physical quantity, synonymous with the total electrification of a body, is not, like heat, a form of energy. An electrified system

has a certain amount of energy, and this energy can be calculated by multiplying the quantity of electricity in each of its parts by another physical quantity called the potential of that part, and taking half the sum of the products. The quantities 'Electricity' and 'Potential,' when multiplied together, produce the quantity 'Energy.' It is impossible that electricity and energy should be quantities of the same category, for electricity is only one of the factors of energy, the other factor being potential." Examples are also given of other factors producing energy. In the present state of electrical science it is perhaps necessary that the two-fluid and one-fluid theories be discussed; too little, however, is said of another theory which looks upon electricity as a substance, an element having a far greater affinity for other matter than fluorine. According to the supporters of this theory—and they are neither few nor feeble—electricity can never be isolated, though the conditions of combination may be changed, giving rise to the so-called positive and negative electricities. We think that Professor Maxwell is entirely wrong in his reasoning on this theory. He says :—"Electricity is treated as a substance, but inasmuch as there are two kinds of electrification which, being combined, annul each other, and since we cannot conceive of two substances annulling each other, a distinction has been drawn between free electricity and combined electricity." If the positive and negative states of electrification consist in the combination with more or less than the normal quantity of electricity, and when combination exists in its normal state we get no signs at all of electrification, this latter does not prove that annihilation has taken place, but that our instruments as yet are not adapted to the detection of electricity in this state. The learned professor might as well state that no other element exists because our chemists as yet have failed to detect a new one. With every improvement in our means of detection we have increased the number of known elements, and we have every reason to suppose that further improvements will further increase the number. Again, this theory does not need two substances, but one; and we think a positive assertion can be made "that no one element can annihilate itself." We can only refer to one other theory which has, so far as we are aware, been relegated by electricians to the regions of mumbo-jumbo—they have not deigned to notice it in any of their elementary books. Professor Challis, writing of electricity, says :—"The mechanical operation of rubbing disturbs the superficial atoms in such a manner that, in one of the substances they are pushed in and in the other they are drawn out beyond their ordinary neutral positions. That these two effects must be simultaneously produced will appear from the consideration that the applied forces operate to displace the atoms by the intervention of the mechanically equivalent atomic and molecular forces of the two substances, and the resultants of these, where the substances are in contact, necessarily act on both in the same directions relative to the common tangent planes at the points of contact, thus compressing one and dilating the other. The two kinds of electricity are called *vitreous* and *resinous*, or *positive* and *negative*. I assume that the vitreous, or positive, electricity corresponds to the compressed state of the superficial atoms." These theories of electricity form legitimate grounds for discussion, which, if of no very great benefit to students who learn only to use, and remain satisfied with the least common measure of knowledge which enables them to gain the place of lineman or superintendent, is not wasted, and ought to serve as an incentive to the student to pay close attention to the phenomena his work may bring him into contact with, and which may serve to settle the whole question. We cannot say the same, however, of that part of text-books which is wasted in defining terms long since proved incorrect; and in explaining phenomena on the most vicious and incorrect principles that empiricism can devise. Why cannot we abolish the terms vitreous and resinous? Why not get rid of the ideas expressed by free and bound electricity? Why not, with all the scientific hammers we possess, get rid of the absurd and childish statement that the earth is a vast reservoir, and that the electricity sent from the rubber of an electric machine is lost in this immense reservoir? Why not? Simply

because our would-be teachers require to be taught. The question of the reservoir ought to have been abandoned after the publication of the first part of Mr. F. C. Webb's work on "Electrical Accumulation and Conduction." The reasoning therein is very strong, and it seems to us incapable of being disproved. The book is now, we believe, out of print. The second part, we are sorry to say, was never published, the reason given being the want of success of the first; and yet there does not exist a better book on this branch of the subject in any language. With such a work in existence, how could authors write—"More of the fluid will be accumulated by further excitation, the ground being the inexhaustible source of the electric fluid;" or, "The earth is a vast reservoir—a species of drain—which sucks up and absorbs at the two extremities of the wire the free electricities which the battery, or any apparatus that is the generator of electricity, sends into it;" or again, "By means of the electrophorus, or rather, by the small quantity of negative electricity resident in its resinous cake, we are thus enabled to 'pump up,' as it were, an indefinitely large supply of electricity from the earth as a common reservoir of electricity?"

Mr. Webb, in the work above referred to, after quoting the explanations generally given, says, "I maintain that these explanations are erroneous, &c.," and devotes five or six chapters to the proof of his statement. And he is undoubtedly correct. The earth is no more a vast reservoir of electricity than its crust is only forty-five or fifty miles in thickness. It seems to us that all text-books and all teachers should pay special attention to the description and discussion of the laws relating to the circuit. This ought and may conveniently be divided and considered under the titles of Conductive and Inductive Circuits. Most authors seem ignorant of the latter of these terms; they write with great facility about "induction," but their plagiarisms show they understand very little about it. Indeed, were this really understood, the absurd nonsense of a "vast reservoir" would not be written. Using the terms positive and negative, we are pretty well agreed upon one fundamental law—that it is impossible to generate any quantity of positive electricity without at the same time generating an equal quantity of negative, and *vice versâ*. There is another equally fundamental law, not, however, so generally stated, even if it be admitted. It is this. Whenever positive and negative electricity are generated, their whole end and aim, their life-long work, is to get together again, and they never cease attempting this till—if one may use the expression—they have done it. Admitting these two laws, the student, and, for the matter of that, the scientist, must be blind who does not clearly see the whole phenomena of induction and conduction. Admitting these laws, we hold that the very idea of repulsion is eliminated from the science of electricity, there is no such thing—repulsion does not, cannot, exist. The whole phenomena of pith-balls, gold leaves, torsion balance, &c, &c., are as easily and much more scientifically explained by means of attraction than by the olla podrida of repulsion and attraction. Mr. Sprague is evidently of this opinion, and we wish he had taken his standpoint on it more decidedly. Speaking of the ordinary theory, he says: "These statements are received as absolute truth by electricians, and upon them the fluid theories of electricity are based; and yet there is scarcely a truth in them which is not overweighted by an error." "At this stage it is proper to remark that, without absolutely denying the existence of electrical repulsion, the experiments on which that doctrine is based are mostly illusory." Singularly enough, he does not in the illustrative experiments he gives, mention one that assists us in understanding the reservation implied by his words, "without absolutely denying" in every case we have "apparent repulsion."

The doctrine of always attraction, nowhere repulsion, seems to be like the fabled red rag when waved in front of the bull—calculated to rise the electric ire of thoughtless pretenders, who say this or that must be true because this or that man amongst the foremost in the scientific world says it is so. It is not the thinkers, but the would-be-thought thinkers who create disturbances in the scientific world. We would ask these meddlers to remember the noble words of a master. Faraday says, in the pref-

ace of vol. 1, "Experimental Researches:" "Other parts of these researches have received the honor of critical attention from various philosophers, to all of whom I am obliged, and some of whose corrections I have acknowledged in the footnotes. There are no doubt occasions on which I have not felt the force of the remarks, but time and the progress of science will best settle such cases, and although I cannot honestly say that I wish to be found in error, yet I do fervently hope that the progress of science in the hands of its many zealous cultivators will be such, as by giving us new and other developments, and laws more and more general in their applications, will even make me think that what is written and illustrated in these experimental researches belong to the bygone parts of science." We certainly do not wish to be found in error, but feel that criticism is necessary upon points which to us seem so palpably incorrect. Once more stating the laws as we recognize them, we will endeavour to explain the so-called repulsion of gold leaves by attraction :—(1) Equal quantities of positive and negative electricities are generated at same time. (2) The great and only object of these electricities is to recombine. (3) No quantity of electricity is ever lost, the earth not being a reservoir sucking up or absorbing. (4) Equal quantities of positive and negative are lost sight of on recombination. When facility is given for a recombination, as when two sides of a Leyden jar are connected by wire, we form a conductive circuit, otherwise we have an inductive circuit. (5) The uncombined electricities always get as near to each other as possible. (6) Positive electricity has absolutely no effect, either repulsive or attractive, upon positive, and similarly, negative has no effect on negative. Let a gold leaf electroscope be charged, say, with positive electricity. The leaves diverge, and are commonly said to be repelled. Now the real action is this: Originally the gold leaves and conductor contained equal quantities of positive and negative electricities. The negative is taken away to combine with positive presented to the knob. But undoubtedly the generation of this positive necessitated the generation of an equal amount of negative electricity. Where, in the name of common sense, is this gone to? Are we to still continue the childish unscientific explanation that it is absorbed by the earth? It cannot be. This negative electricity has followed the insulated positive and has been accumulated upon the nearest conductor, whether that consists of the operator's body, the walls of the room, or the earth. When the positive was brought nearer to the electroscope than to the original negative it was combined with, it inductively separates the normal electricity in the leaves and conductor, ultimately by the conditions of the experiment it combines with the negative of the electroscope, and on its combination is lost to our senses and instruments. But how about the remaining electricities? The leaves diverge not because of repulsion but because the freed negative electricity has found out, so to speak, the freed positive electricity, and wishes to combine with it. According to our idea the two uncombined electricities get as near to each other as possible, and as every good electroscope has generally slips of tin-foil on its sides, the uncombined negative electricity is accumulated upon these strips, and it is owing to this accumulation that the gold leaves diverge. The positive electricity in the leaves and the negative electricity in the strips of foil act on each other, causing the phenomena. The strips, however, are not necessary—the phenomena occur just as well without as with them; if not present, the electricity accumulates upon the nearest conductor, acting just as effectively.

THE THAMES INUNDATIONS.—Recently, at a meeting of the Metropolitan Board of Works, Mr. Roche introduced a draft Bill in relation to the prevention of inundations from the Thames. He stated that the principle of the Bill was, that the local authorities, as defined in the Metropolis Local Management Act, 1855, should be responsible for the raising of embankments, and doing all such works as in the opinion of the Board were necessary to prevent inundations, the Board being empowered, in case of the default of a local authority, to do the work and charge the local authority with the expense. After some discussion the preamble was agreed to.

ON FLUID COMPRESSED STEEL AND GUNS.*

By Sir JOSEPH WHITWORTH, Bart.

In the present paper the writer does not propose to enter into the consideration of the various chemical effects produced upon steel, but to confine himself to what has a more immediate and practical bearing upon the application of steel as a constructive material. The difficulty he experienced in obtaining sound, ductile steel, led him to institute experiments in compressing the steel while in a fluid state.

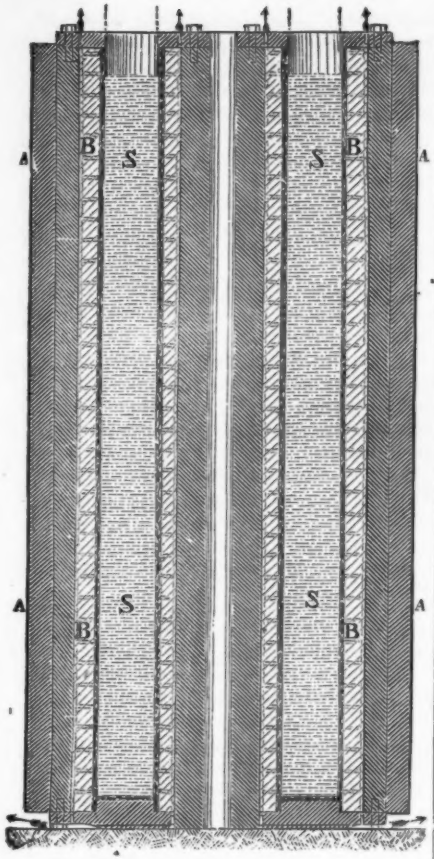
For melting steel there are in operation at his works, in Manchester, the crucible, the Bessemer and the Siemens-Martin process; and pressure is applied to the fluid metal so melted, in each case, as quickly as possible after it leaves the furnace. The use of crucible steel for constructive purposes is being rapidly superseded by the metal produced in Bessemer converter or the Siemen's furnace; and although crucible steel is yet occasionally specified and demanded on account of its supposed superiority, yet its superior quality, when it exists, will be found to be due to the use of purer and better materials in its production; and if equally pure irons be employed in the other processes mentioned, a product is obtained equal in strength and ductility to crucible metal. Various specimens are exhibited of fluid compressed steel, which have a tensile strength of forty tons per square inch. The cylinders which have been burst by successive discharges of gunpowder, do not fly into pieces, but open out and tear like paper.

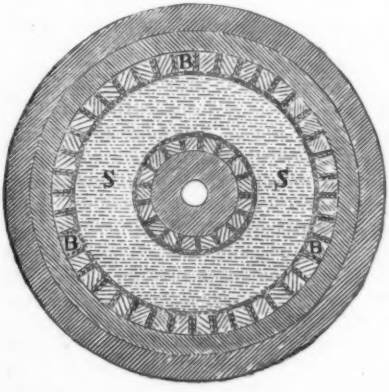
The power of elongation, represented by the word "ductility," is of the first importance for some purposes, as in guns, torpedoes, boilers, etc., and wherever severe strains might be suddenly applied; while in some cases, as in cutting tools, the strength of the metal is of the first importance. Cylinders of steel, to resist with safety the strains produced by gunpowder, should have a ductility of thirty per cent.; more than this is unnecessary, for cylinders of such metal do not fly into pieces when burst, but simply open out or tear like paper, and a metal of greater ductility would not, therefore, be required for structural purposes. It is now possible to produce with certainty, by the compression of fluid metal, steel that will bear a tensile strain of forty tons per square inch, which elongates thirty per cent. of its length before breaking. Such a metal would not harden sufficiently to cut other metals.

Compressing.—The mould box for fluid compression, shown in Figures 1 and 2, has an outer hoop of steel A A of the necessary thickness to withstand the pressure. The inside of this hoop is lined with a layer of cast iron lags B B, from the front to the back of which are a number of grooves or channels, by which the gases can reach the outer face of the lags between them and the inside of the steel box; while communicating with these transverse channels are made a number of vertical or longitudinal channels, which open to the atmosphere at the top and bottom of the mould, and from which the escaping gases continue to burn for some time, when the pressure is applied to the fluid metal S S. The inside of the mould is finally lined with a layer of refractory sand, which, while protecting the cast iron lags from fusion by the heat of the melted steel, also permits of the gases being driven through it by the pressure to the back of the lags, and so to the asmosphere. The case is built up similarly to the mould.

Dr. Tyndall, in a lecture recently delivered at the Royal Institution, said of this process, witnessed by him: "A large ladle was at hand, and into this was poured the molten metal from a number of crucibles. From the ladle, again, the metal was poured into the annular space just referred to, filling it to the brim. Down upon the molten mass descended the plunger of a hydraulic press. On first entering it a shower of the molten metal was scattered on all sides; but, inasmuch as the distance between the annular plunger and the core on the one side, and the sheath on the other, was

* A paper read before the Society of Mechanical Engineers.





only about one-tenth of an inch, the fluid metal was immediately chilled and solidified.

"Thus entrapped, it was subjected to pressure, which amounted eventually to about six tons per square inch. Doubtless, gases were here dissolved in the fluid mass, and doubtless, also, they were mechanically entangled in it as bubbles. I figure to myself the fluid metal as an assemblage of molecules, with the intermolecular spaces in communication with the air outside. Through these spaces I believe the carbonic oxide and the air to have been forced, finding their escape through the porous core on the one side, and through the porous sheath on the other. From both core and sheath issued copious streams of gas, mainly it would seem in the condition of carbonic oxide flame. A considerable shortening of the fluid cylinder was the consequence of this expulsion of gases from its interior. The pressure was continued long after the gases had ceased to be ejected; for otherwise the contraction of the metal on cooling might subject it to injurious internal strains. In fact, castings have been known to be rent asunder by this contraction. By the continuance of the external pressure, every internal strain is at once responded to and satisfied, and the metal is kept compact."

Forging.—The steel castings are forged by either the steam hammer, the rolls, or the hydraulic press, or a combination of these; but for large forgings generally, there is a great superiority in the work produced by the hydraulic forging press. For the stroke of the press is that of a continuous pressure, and it is effective right through the mass of metal; whereas, the blow of the steam hammer is largely expended within a short distance of the surface, while the center of the work is for a certain period comparatively unacted upon, and therefore the different parts of the metal of the forging, produced under the hammer, exist in very different molecular conditions. This is not the case, as before stated, when the forging press is employed.

At the writer's works, in Manchester, will be seen a number of fluid compressed castings and forgings; one, a screw propeller and shaft, which, when finished, will be eighteen tons weight, to sup-

ply the place of an iron shaft weighing thirty-one tons, thus saving the rotation of thirteen tons during the life of the engine. Also, number of hoops from four to eight tons weight for twenty-two and thirty-five ton guns; also in vessels for torpedoes, etc., and steel linings of various sizes for marine engine cylinders, some six feet diameter, four feet nine inches long, and 1⅝ inches in thickness.

Fluid Compressed Steel.

TENSILE STRENGTH AND DUCTILITY OF DIFFERENT QUALITIES.

Tensile Strength Tons per sq. in.	Ductility or percentage of Elongation.	Purposes for which the Steel is available.
Tons.	Pr. ct.	
40	32	Axles, Boilers, Connecting rods, Cross-heads, Crank-pins, Hydraulic Cylinders, Locomotive and Marine Cranks, Propeller Shafts, Rivets, Railway Tires, Guide Screws, Gun Furniture, Gun Barrels, Air Vessels for Torpedoes, Carriages for Field and Naval Ordnance.
48	24	Cylinder Linings for Marine Engines, Slide Bars for Locomotives, Shafting, Couplings, Lathe-Mandrils, Drilling Machine Spindles, Eccentric Shafts for Purchasing and Shearing Machines, Pillars for Hydraulic Presses, Large Swages, Pressure Blocks for Machines, Hammers, Hoops, Trunnions for Ordnance.
58	17	Large Planing and Lathe Tools, Large Shears, Drills, Smith's Punches and Dies and Sets, Small Swages, Cold Chisels, Screw Tools, Corn-mill Rollers, Armor-piercing Shells.
68	10	Boring Tools, Finishing Tools for Planing and Turning.
72	14	For particular purposes.

Mr. Siemens said: In the plan now carried out, the steel after it had been produced was dealt with under a method entirely different from those before adopted, being here compressed while in a fluid or semi fluid state. He had at first felt considerable doubt as to the effect of the new method. It was said that in applying hydraulic pressure upon fluid steel the gases contained in the fluid metal would be driven out; but he could not see how that was to be done by mere pressure. For in applying pressure to a fluid, the pressure acted in all directions equally; and why a particle of gas held in suspension in the fluid should go in one direction rather than another, and should get away from the pressure to which it was subjected, it seemed difficult to conceive. The facts, however, spoke for themselves; and these being ascertained, it was more easy to find an explanation of what took place. The result, he suggested, might be accounted for by the circumstance that the fluid steel, congealing first on the outside of the mould, offered more resistance there to the motion of the plunger, and the outside thus became, comparatively speaking, porous, while the fluid portion in the center received a larger amount of compression than the outside which had more power of resisting the pressure. The particles of gas entangled within the fluid mass would, therefore, encounter rather less resistance towards the outside than towards the inside, the full hydraulic pressure being transmitted to the center of the fluid mass. In that way the expulsion of the gases from the fluid metal might, perhaps, be accounted for; and he should be glad if Sir Joseph Whitworth would give his explanation of the matter. The fact being admitted, it was clear that the steel produced by that mode of treatment must possess many great advantages over metal treated in the ordinary way by hammering; for it was hardly to be supposed that hammering would be capable of driving out the gases.

With regard to the mode in which these gases entered the metal, he did not think they were merely entrapped mechanically at the time of pouring out the metal into the mould; because, in working melted steel in the open hearth of a regenerative furnace, he had found that the metal could be made at any moment to evolve gases in great quantities by simply plunging a cold bar of iron to the bottom of the fluid mass. The fluid metal evidently absorbed carbonic oxide to a very great extent; and it was due to the partial congelation of

the metal that the gases were suddenly set free. Similarly, in the Bessemer process, a great ebullition took place on pouring the fluid metal out of the converting vessel into the iron moulds, and the top of the moulds had to be closed by a stopper to prevent the metal being thrown out by the ebullition. It was clear, therefore, that the metal contained a large quantity of gas occluded within itself; and if this was retained in the metal it became a source of weakness. However small the bubbles of gas might be, their presence would have the same effect as the presence of foreign matter between the particles of metal, and must necessarily weaken it.

Mr. E. A. Cowper, referring to the condition of the gas in fluid steel under pressure, and the condition of the compressed steel after the pressure was removed, remarked that a certain quantity of gas must come from the sand lining, as in ordinary casting, some of which was carbonic oxide. Some of the gas, however, he thought still remained in the steel, and was accordingly subjected to the combined effects of the high temperature and high pressure in the mould. Supposing a very small cavity in the fluid steel were filled with this gas at atmospheric pressure, it would first be expanded about seven times by the heat of the melted steel, and then being put under the pressure of six tons per square inch, or 900 atmospheres would be compressed to 1-900th of its size when heated; and in the subsequent cooling the pressure of the gas contained in it would be reduced again seven times, leaving it at a pressure of about 1,920 lbs. per square inch, within an infinitesimally small bubble. The difference of the temperature of the gas when the steel was in a melted state and when it was cool was very great; and the difference of pressure was largely accounted for by the expansion of the gas, and it was possible it would be a very small speck when compressed under the hydraulic pressure of six tons per square inch. He thought the gas must remain in this state in the steel ingot.

As there was carbon present in the steel, and it was almost impossible to avoid oxydizing some particles of the iron in the Bessemer process, these particles were together in an ingot and would continue puddling after being run into the mould. The union of the carbon and oxygen of the iron formed carbonic acid, causing the ingot to froth up in the mould, and producing a sponginess in the top of the ingot.

Sir J. Whitworth remarked that, when the pressure of six tons per square inch was applied to the fluid metal in the mould, a column of fluid metal of eight feet height was reduced one foot in less than five minutes. No doubt, there was a great deal of gas expelled during the compression, but he believed nine-tenths of it was common air; there was a portion of other gas mixed with the air, because it was burning while the pressure was on, but the greater portion he considered must be common air.

It had been pointed out that in casting ingots of steel in the ordinary way, the metal was sometimes sound and sometimes not sound. His own experience had been that steel castings possessing twenty-five, thirty or thirty-five per cent. of ductility or power of elongation, when pulled asunder were never found to be sound; with ten or fifteen per cent. of ductility an ingot might be sound through three-fourths or seven-eighths of its length from the bottom end; but he had never got it sound when the ductility was higher. The great value of a metal lay in its tensile strength and ductility combined. The best metal for guns, torpedoes and boilers was that which had a tensile strength of forty tons per square inch, and had also thirty per cent. of ductility. The effect of this was that when it was burst, it simply opened out, and therefore there was no danger. There was never much more than thirty per cent. of ductility in the compressed steel; in Low Moor iron, forty per cent. was obtained, which was about the limit practicable. It was impossible to get both high tensile strength and high ductility, because as one was gained the other one was lost. In the case of the metal having a tensile strength of forty tons per square inch, and thirty per cent of ductility, these two figures together amounted to a total of seventy; and it was a great achievement to get so high a total divided in such nearly equal amounts between the two qualities of tensile strength and ductility.

Mr. W. Carpmael said he had seen in operation the process of compressing fluid steel, and certainly the apparent effect was that of squeezing the gases out of the metal; and looking at the porous character of the mould, and the fact that its surface was covered with burning gases during the time of compression, he did not see much difficulty in believing that the gases really were expelled by the pressure; just as, if the mould were filled with wet sponges, there would be no difficulty in squeezing the water out of them in the same way. Or, he thought, the operation might be properly represented by considering the steel as holding carbonic oxide gas in solution; this gas tended to escape as soon as sufficient pressure was applied, and where the fluid metal came against the sides of the mould it parted with the gas from its surface. The surface of the metal being thus purged from gas took a further supply from the interior portion of the mass, and parted with it in the same manner; and in that way he saw no difficulty in believing that the gas could be given off during the time that the metal remained in a liquid state under the pressure in the mould.

Mr. H. Davey suggested that the great increase of strength in the metal, which had been compressed while in a fluid state, might arise not from the air being squeezed out of the fluid mass, but rather from a different arrangement of the particles. For when steel was heated and immersed in cold water, the increase of strength thereby produced was very probably due simply to a different aggregation of the particles; and it seemed to him possible that, when steel in a fluid state was subjected to such a heavy pressure, it might in cooling assume a different molecular condition from that which it would assume under a much less pressure, or under the pressure of the atmosphere only.

Mr. F. W. Webb considered the subject of the paper a very interesting one, as he believed steel would be the most important metal in the future of this country. In reference to casting steel sound without pressure, he had found that the "piping" and bubbles often met with in steel castings were due to a want of knowledge in putting together the moulds, so as to provide for the escape of the gas without injury to the castings. Where the mould box was divided into four or five parts for making a long casting, if care was not taken to lute the partings in the mould as fast as the metal rose to each parting, it was found that wherever the gas blew out there was line of bubbles in the casting from the center to the outside. At Creusot, recently, he had been very much struck with the magnificent ingots cast there from the Martin-Siemens furnaces, completely free from flaws, only a small head being left on the top, about six inches diameter and four feet high; he had seen an ingot split, and there was not the slightest sign of honeycomb. The head of the metal had a great deal to do with making sound steel castings. In 1872 he had shown at the Kensignton Exhibition a steel ingot cast at Crewe without pressure and perfectly free from honeycomb; that was done with a four-foot head, care being taken that the mould was perfectly dry and made of a porous non-conducting substance. Large steel shafts also had been cast partly to shape in moulds of a non-conducting substance, and had been so sound that he had seen them turned after forging perfectly bright and without a speck on their surface. For wheels, pinions, and housings, not requiring any forging, the addition of a little phosphorus was of a great help in obtaining clean castings; if the steel contained one-third per cent. of phosphorus it was much sounder than without it.

With respect to ductility, he had found no difficulty in getting mild cast steel that possessed as much as thirty-three or thirty-four per cent. of ductility, its tensile strength being from thirty to thirty-two tons per square inch. He had made now 11,000 tests of steel boiler plates of this quality, by punching a five-eight inch hole in a strip three inches wide, and then driving a drift one and a-half inches diameter through the hole cold, without cracking the strip. This showed that it was possible to get a uniform quality of steel of high ductility. Out of 500 sets of steel boiler plates he had not had to condemn more than a single plate, and that was condemned through want of annealing;

proper annealing was indeed indispensable for making steel boilers. The steel made at Crewe Works for the boiler plates contained one-fifth per cent. of carbon, the pig iron used being not too silicious; and it was considered a favorable sign if a little swell was left at the bottom of the ladle in casting the ingots. The exterior of the ingots showed a number of small air holes; but when an ingot eighteen inches square had been hammered, reheated and rolled down into a plate three-eight inches thick, these surface defects disappeared practically, being reduced to a mere scale. After the shearing and punching was done, the plates were finally annealed before making the boilers; and he had never known an instance of failure with a boiler so made. For fireboxes, steel could hardly be tempered so low as was desirable, and he thought to use steel for the firebox with an iron shell was to begin at the wrong end; he considered it preferable to employ Bessemer metal made of ferro-manganese instead of spiegeleisen. However, the steel boiler with steel firebox and tubes which had been shown at the Kensington Exhibition in 1872 was still running; and not having heard of it since, he believed it was working all right. Five more fireboxes had been made with ferro-manganese, care being taken in all of them to keep the plates as thin as possible, on account of their being in contact with the fire, and to have no double thickness. The sides, back and front of the firebox were a single plate, which for the largest firebox was five feet eight inches wide, twenty-two feet long and five-sixteenth to one quarter inches thick, this was doubled up to the required shape, leaving only the tube-plate and top-plate to be riveted in, the thin plate under the tubes being flatted back into the water space to receive it. The firebox plates were made to a tensile strength of only twenty-eight tons per square inch, so that they were practically wrought iron; after the first rolling they were immersed in a water tank twenty-four feet long and six feet wide, which was close in front of the rolls, and the plates became buckled up in every way; they were then reheated, this treatment being repeated four times. The plates appeared more like morocco leather all over the surface, and after shearing they were finally annealed, until they would stand bending double when cold without cracking.

Sir J. Whitworth said he had not made any trial of applying pressure to cast iron in a fluid state. It should be distinctly understood that the value of the process of compressing fluid steel under the heavy pressure employed was not in proportion to the ductility alone of the metal so produced, but consisted in obtaining high tensile strength in combination with ductility. He had himself never met with a sound steel ingot, cast in the ordinary way, that had anything like thirty per cent. of ductility; and he should be glad to know whether ingots, forged simply as cast, and possessing that degree of ductility, had ever been cut in two and found free from air cells. Generally, to get rid of air cells, the pressure of six or eight tons per square inch upon the fluid metal was sufficient, and in practice no higher pressure was used, in order to avoid subjecting the mould boxes to too severe a strain. He had, however, applied a pressure of as much as twenty tons per square inch, in order to see what result would be obtained; and had found that, with that amount of pressure, the fluid compressed steel was rendered so perfect that it could not afterward be improved by any process whatever, and needed no forging for any purposes. Instead of casting the fluid compressed steel in solid ingots, he considered it much preferable generally to cast it in the form of tubes, so as to afford greater freedom for the gases to escape from the inside surface as well as from the outside. No one had done more than Mr. Webb in connection with the use of steel, but it had been in dealing principally with boiler plates, rails, and axles, for all of which the cast ingots necessarily underwent a great amount of forging and rolling; and the soundness of the original ingots was therefore of less consequence in such cases.

Mr. G. F. Deacon thought it was impossible that expulsion of the gases from the interior could take place by compression while the metal was in the fluid state, because the pressure would be equal, or nearly equal, in all directions. The pressure, it appeared, was sufficient

to reduce the ingot to seven-eighths of its original length, and it seemed to him that the expulsion of the gases, so far as it was due to compression, must really commence with the process of congelation, and that it took place by reason of the same kind of action that had occurred in certain rocks in which planes of cleavage having no relation to stratification, had been produced by compression. Supposing such planes to form at right angles to the direction of pressure during the process of congelation, the bubbles of gas would be flattened, and joining each other, would find the least resistance in a horizontal direction; thus, after the commencement of congelation, but not before, there would be some analogy to squeezing water out of a sponge. If that view were correct, it would suggest the possibility of there being a certain amount of lamination in a mass compressed in one direction in this manner, after the commencement of congelation, and during the lateral expulsion of the gases. He should be glad to know whether there was not some difference in the tensile strength of the metal in the two directions, at right angles and parallel to the direction of pressure; if so, the greater strength being round the gun would be in that direction in which it was most required.

Mr. D. Adamson considered the results of the process of compressing steel in the fluid state, as described in the paper, most interesting and valuable; and he thought the soundness of the ingots so compressed could be sufficiently accounted for on mechanical principles, without having recourse to any explanation involving chemical action for the expulsion of gases. For it was impossible to get a solid ingot in casting in an ordinary iron mould; the metal in running into the mould must necessarily become cooled and solidified on its outer surface first, and by its contraction caused the metal to boil out at the top after being run, the center still continuing fluid. But as soon as the ingot had become solidified, and fixed at the circumference and at the ends, there was no longer any possibility of getting it solid at the center. The outer surface being solidified, the natural contraction of the metal in cooling must leave some vacuous pores. The metal was then no longer of a weldable character, and, however solid it might be made to appear by subsequent hammering, the mass was not thoroughly united, particle to particle, but only mechanically closed together. The difference between the tensile strength and ductility of the compressed steel was explained on that basis, the compression taking place during the time that the outer surface was becoming solidified, and thus the whole mass was made really solid. In Sheffield, he believed, it had long been understood amongst the makers of cast steel that, in order to cast a solid ingot, a stick of pine wood would be held in the center of the melted steel in the mould, and lifted out just when the metal was setting; by this means the center portion was kept liquid to the very last, and enabled to set with a better chance of being solid; and he understood the same plan had many years ago been found by Mr. Mushet most efficacious in preventing the metal from becoming separated at the center. The steel made at Crewe, possessing a tensile strength of about thirty tons per square inch with thirty per cent. of ductility, was no doubt entirely satisfactory for the purposes for which it was employed; but the great point was that in the compressed steel with the same ductility the tensile strength was increased to forty tons, on account of the particles being really welded together in the interior of the mass. With this metal for the plates of ships there would be a great reduction in weight, allowing of a corresponding increase of cargo

Mr. H. Sharp observed that in the ordinary mode of casting Bessemer steel ingots, by pouring the metal into open cast-iron moulds, there was no doubt that perfectly solid ingots could not be obtained. But he had seen many Bessemer ingots which had been cast under the pressure of a head of metal of from four to five feet height, which were perfectly sound; and in making large castings for forgings, the plan he adopted was to use a loam mould, and give the casting a head of the height named, by which means the portion required for the forging would be perfectly sound and free from honeycomb. Although these castings were not made under such a pressure as the steel made by Sir J. Whitworth's process, still they were very

strong. The ductility of the steel in the ingot he had not ascertained, all the tests having been made after forging it or rolling it into plates; in this state it was found to have a tensile strength of thirty-three to thirty-four tons per square inch, with twenty to twenty-five per cent. extension before breaking, the trial pieces being six inches long, and stretching 1¼ to 1½ inches before breaking.

Mr. C. Cochrane was somewhat surprised to learn that the ingot of fluid compressed steel was reduced in bulk to the extent of so much as one-eighth by the pressure put upon it in the mould. This, he thought, could not all be owing to expulsion of the gas, as he could not imagine the ingot had originally contained so large a quantity of gas. It appeared to him the reduction would partly be due to the expansion of the heated mould, which must be materially increased by the very heavy pressure; but how much would thus be accounted for, it was difficult to say. To some extent the result of the compression seemed to be more of the nature of prevention than of cure; the gas between the molecules, being compressed to 900 atmospheres, would be further dissolved in the steel, but what was not so dissolved would be prevented from escaping.

Mr. E. Reynolds remarked, in reference to the application of the process to other metals than steel, that the system had been extensively used in copper works for the last twenty years at least; this, however, did not detract from the merit due to the successful application of a difficult process. He could confirm the statement that perfectly sound steel ingots were produced, though perhaps not so regularly as could be desired, without any greater pressure than the usual head of metal; one necessary condition being that the steel should be, as had been said, "dead melted." If it were not so, the steel would "rise" in the moulds, as was very commonly the case with Bessemer metal, from the evolution of gases within the metal. It was common to "Stopper" Bessemer moulds, in order forcibly to confine this rising within small limits; and considering how small a force was sufficient in this instance to resist an apparently powerful agency, it was easy to understand that a greater force applied by a press might wholly prevent the internal evolution of gas, and to conceive that it might cause re-absorption of such as might be partially developed.

But good work might be done, without the use of a press, by "dead melting," and it was desirable to trace the nature and effect of this. In melting by the crucible, it was not sufficient to get the material well heated and thoroughly fluid; but after it had arrived at that condition it must be left for sometime in the furnace to "kill" it. In a lecture upon fuel delivered by Mr. Siemens at the Bradford meeting of the British Association, attention had been called to the remarkable discovery that there was a limit to the heat at which combustion was possible; and that this limit, at which oxygen and hydrogen might be mixed without burning, which it had been proposed to call the heat of dissociation, had been fixed by M. St. Clair Deville at about 4500° Fahr. The application of the same principle to the combination of oxygen with carbon, silicium, etc., appeared to furnish an explanation of the nature of "dead-melting," which might assist in the more general production of sound ingots. In the ordinary methods of steel-making a great heat was obtained, and in the Bessemer process the heat was still greater; so that in either case a near approach to the heat of dissociation was soon reached, and chemical action was arrested before being completed. But if the metal were cast in that state, it would soon cool down to a temperature at which chemical action would again come into operation; and gases would be evolved, giving rise to honeycombing. The remedy was to keep it melted for a considerable time, so that the chemical action, which was then very languid from the near approach to the heat of dissociation, might have time to become complete.

An instructive illustration of this principle was afforded in the working of the regenerator tank furnaces for steel making. If these furnaces were worked at too low a heat, the charge would become softened so rapidly that the metal would set (soft steel having a high melting point), and much time and patience and a great heat would be necessary to remelt it; and inasmuch as

the intense chemical action, due to low heat, applied to the furnace as well as to the charge, the slag would run into the sand bed of the furnace as into a sponge, and when remelted probably a considerable part of the charge would leak out. This made it necessary to take from ten to twelve hours for a process which might otherwise be completed in three or four hours, and to use a very high heat in order to avoid cutting the furnace to pieces. The fact which had been mentioned, that if an iron bar were put into the melted metal in the furnace it caused gas bubbles to arise, was, he thought, because the temperature was thereby lowered sufficiently for the chemical action to recommence; and if it was attempted to rabble the metal, an excessive ebullition was caused.

When steel really dead melted was cast into a mould, it naturally cooled first on the outside; and the inside having to contract afterwards, a cavity was left in the center of the upper part, which was called "piping," the remainder of the ingot being sound. Though in the present state of the manufacture the system of compressing the metal whilst fluid might be the most certain method of preventing the re-commencement of chemical action, and might therefore be of great value for the highest class of work, its legitimate function ought to be confined to forging down the metal whilst cooling, and to reducing the loss by piping. Any apparent increase of strength he believed would be found to be the result of the forging action; and if the ingot was sound, it would be equally obtained by forging it afterwards in the usual manner.

Mr. C. W. Siemens concurred with the remarks of Mr. Reynolds respecting the dissociation that took place at the high temperature which existed in a steel melting furnace. He added that, on putting a cold bar into the fluid metal in the furnace, the ebullition took place only during the earlier part of the process; after the steel was completed and spiegeleisen or ferro-manganese was added, no ebullition was caused by the insertion of the bar, showing that at that period of the process the point of dead-melting had been reached.

Mr. F. Preston mentioned that an illustration of the importance of getting rid of the air, for obtaining good castings in steel or iron, had been afforded at a foundry where there were three cupolas, one of which was never successful in making good castings, and in running the metal the gas was always given off like fire-works. On taking this cupola down, it was found that it had been erected on masonry with a large circular cast-iron bed-plate upon the top, three or three and a half inches thick, forming the bottom of the hearth. The reason of the previous difficulty was then obvious; the plate had prevented the air from escaping through the bottom of the hearth, and it was as necessary that the air should have vent when melting the iron as when making a casting. The cupola was afterwards rebuilt without the iron plate, and then produced iron like that from the two other cupolas; the plate had been the only cause which had prevented the air escaping at the base, and after its removal the escape was so strong that the gas could be lighted.

The adoption of a porous mould lined with loam for casting the ingots of fluid compressed steel was in accordance with the universal experience in making large moulds for ordinary castings. In a moulding shop not only was ample provision made for the escape of the air from the mould through numerous rents, but a straw or hay bed was made for the mould to rest upon, in order to give further facility for the air to escape through the bottom of the mould. If the same attention was given to casting steel ingots as in making ordinary castings, it would be found that the air would be got rid of far better than by casting in iron moulds; but if an iron mould was lined with ganister, the principle of a porous mould was better carried out. Another great objection to the usual mode of casting the ingots was that the iron moulds were let upright, with the broad end forming the base to stand upon, which prevented the gas from escaping freely; the moulds ought to be set with the wide mouth upwards, to encourage the gas to get away.

Mr. F. W. Webb said the trial made of casting in a vacuum had been successful as regarded the production of a vacuum, for the pipe connecting the air-pump was pumped full of steel; but it did not result in a sound ingot. The

idea was brought out eight or nine years ago by Messrs. Lüthy and Bell at the Bolton Iron and Steel Works.

Sir Joseph Whitworth said with regard to annealing, in the manufacture of Krupp Steel more carbon was put into the metal than the steel was intended to have ultimately, in order to get it sound, and all the forgings were afterwards subjected to a long process of annealing, which was very necessary on that system. But with the fluid compressed steel all the large forgings were completed without even being annealed afterwards; the metal was right already, and required no annealing.

BOILER INCRUSTATION.*

By Mr. F. J. ROWAN.

A FEW years ago the attention of all concerned was exclusively directed to Incrustation, its evils and prevention. The evils produced by it are numerous, for boilers when coated with crust quickly accumulate layers of this material, which is a bad conductor of heat, and thus are not only hard to steam, requiring a large excess of coal, but are more quickly worn out and sometimes suddenly oxydized or "burned" in consequence of the increased temperature rendered necessary in the furnaces. Dr. J. G. Rogers, of Madison, United States, in a paper published some years ago, estimated the conducting power of crusts as compared with that of iron as 1 is to 37.5. A scale 1-16 inch thick, he says, requires an extra expenditure of fifteen per cent. more fuel, and this ratio increases as the scale is thicker. Thus, when it is ¼ inch, sixty per cent. more fuel is needed; ½ inch, 150 per cent., &c. The temperature of the heating surface of the boiler must be raised in proportion to the thickness of scale. Thus, while to produce steam of a pressure of 90 lbs., water must be heated to about 320° Fah., and this can be done in a clean boiler with ¼-inch plates by heating the boiler surface to about 325°; if ¼-inch of scale intervenes between the shell and the water, it will be necessary to raise the temperature of the heating surface to about 700°—almost low red heat. Iron oxydizes the more rapidly the higher the temperature at which it is kept, and at any heat above 600° it very soon becomes granular and brittle, and is liable to give way under pressure. This condition predisposes the boiler to explosion, and makes expensive repairs necessary, and the presence of scale also renders the raising and lowering of steam slower.

The proper circulation of the water is also interfered with by the presence of crust. Both economy and durability thus require the absence or prevention of crust.

There are two distinct classes of boilers which are subject to incrustation, and these are:

(1) Land boilers using natural fresh waters, and
(2) marine boilers using sea water.

(1) There is no doubt that the quality of natural fresh waters varies between wide limits, from rain water on the one hand, which contains no mineral impurities, to that of highly-charged mineral and chalybeate springs on the other.

An examination of the analyses of waters supplied to the principal manufacturing towns in Britain (such as, for instance, are published in the Report of the Registrar-General), and comparison with that of rivers in other countries, demonstrate that no better general or average illustration of this class can be met with than is afforded by the River Clyde water, which was in general use in Glasgow and neighborhood prior to the introduction of Loch Katrine water, and which is still used in some manufacturing establishments. As analyzed by Dr. Wallace in 1848, that water contained the following impurities, which are here tabulated in grains to the gallon:

* A paper read before the British Association at Glasgow.

CLYDE WATER AS SUPPLIED TO GLASGOW IN 1848.

Carbonate of lime	2.52
Carbonate of magnesia	.72
Sulphate of lime	26
Sulphates of potash and soda	1.94
Chloride of magnesium	.40
Chloride of sodium	.54
Oxyde of iron	trace.
Phosphate of lime and alumina	.31
Silica	.28
Organic matter	.89
Total	7.86

Dr. Wallace (to whose courtesy I am indebted for the above and other information on this subject, and for much valuable assistance in connection with this paper) has informed me that the total amount of solid matter in solution in this water increased in quantity gradually, and that in 1854 it amounted to ten or eleven grains per gallon.

The incrustation formed upon ordinary steam-boilers, working in mills at from 15 to 20 lbs. per square inch pressure of steam (above the atmosphere) and using that water, analyzed by the same authority, is found to consist of:

	Per Cent.
Carbonate of lime	66.00
Magnesia	6.05
Sulphate of lime	4.28
Water, with traces of carbonic acid	8.72
Oxyde of iron, alumina, and phosphate of lime	5.85
Silica	8.10
Organic matter	1.00
	100.00

This crust, which is of a dark brown color, and is hard, forms rapidly on the interior of the boilers, and is difficult to remove. But it has been found that by a moderate use of soda-ash this formation is readily stopped or held in check.

The quantity of soda ash used in a pair of boilers (one 30 horse-power, 6 feet 6 inches diameter by 21 feet long; one 40 horse-power, 7 feet 6 inches diameter by 27 feet long) at the works of Messrs. P. Bogle & Co., at Barrowfield, which together require about 9,700 gallons of water per week, is six lbs. per week in both boilers. This is dissolved in water, and fed into the two boilers once a week.

The action of this soda-ash or carbonate of soda, under these circumstances, is a very interesting one, though perhaps not well understood by those using the substance in this way. Sulphate of lime is decomposed by its means and precipitated as carbonate, while a soluble sulphate of soda is formed. The neutral carbonate of lime is likewise produced by reaction from the bicarbonate in solution, and, as thus formed, it will not adhere to the boiler surfaces, but separates as a loose powder or mud, which can be blown out of the boilers or otherwise removed as sludge. In those boilers under notice the quantity of this sludge is found to be three pails full from each boiler every three months.

It has been found, however, that where the neutral carbonate of lime is produced slowly by the action of heat—which drives off part of the carbonic acid from the bicarbonate existing either in solution in the water or as a solid already deposited upon the boiler surfaces—that in this case the neutral carbonate possesses the property of being able to adhere firmly of itself to the boiler plates. It seems to be in this case partially crystalline. Thus the special advantage arising from the employment of soda ash is that it decomposes the bicarbonate rapidly, probably because of the presence of some soda uncombined in the ash, and that the neutral carbonate is precipitated as a loose powder, which will not adhere unless fused or agglomerated by means of some other substance.

Concerning this latter point, M. Bidard, of Rouen, has informed me that his numerous examinations of boiler incrustations have demonstrated the fact that the presence of organic matter is necessary for the formation of boiler crusts which consist essentially of carbonate of lime. Such crusts he has produced artificially, in order to verify his theory. Fresenius, however, quoted by Dr. Wallace, without specially noticing the presence of organic matter, has attributed a cementing property to sulphate of lime, which he always found present in boiler crusts. M. Bidard's explanation thus applies specifically to those crusts which contain carbonate of lime and no sulphate, for it is probable that where sulphate is present it possesses agglomerating power of itself sufficient to render the presence of organic matter unimportant in these instances. This is

demonstrated in examples, in some marine boilers for instance, where crusts are formed without organic matter being present in appreciable quantity, of which the analyses by Dr. Wallace are specimens.

But M. Bidard's remarks show that soda ash might be used with muddy water, such as canal water for instance, and yet a hard incrustation would form. In such a case there would be no preventive but the use of a filter for all the water passing into the boiler in addition to the use of soda ash there.

The use of too much soda ash is injurious in its effects, as the excess boils up and passes over in the steam to cylinders and pumps, where it clogs the piston and otherwise interferes with proper working by making combinations with the oils and greasy matters employed in the machinery. The lavish use of oils and grease of course intensifies this action where it is present, and it has been found that the carbonate of lime itself has passed over from the boiler with the steam, and has entered into combination with the grease where enough was to be found.

All that is needed, however, in working with soda ash is a little intelligent care, and as the matter is simple, a system of working is soon arrived at. As an example, I take the boilers already mentioned of Messrs. Bogle & Co., at Barrowfield. Under ordinary circumstances the manager, Mr. Keith, proceeds, as I have said, blowing out the boilers once in three months. When, however, there is a fresh or "spate" in the river the quantity of inorganic solids in the water is proportionately less, and the quantity of soda ash introduced is in consequence diminished. Muddiness in the water seen in the gauge glasses is a sure test if too much is being used, and when this is noticed and acted upon no inconvenience from material passing over to the cylinders is found to result.

An interesting fact may be mentioned in connection with this part of the subject, and as illustrating how calcareous waters sometimes contain soluble matters which themselves counteract their crust-forming ingredients. The boilers of Messrs. G. & A. Harvey's engineering works in McNeill Street, use the water drawn from the Clyde at a point which is considerably below Barrowfield, the water being contaminated by the refuse from print works and other factories, discharged into it between the two points.

A comparative estimation, by Dr. Wallace, of the qualities of the water from both localities is here tabulated:

	I. From Barrowfield.	II. From McNeill Street.
Total solids per gallon	8.96	15.12
Insoluble in water, carbonates of lime and magnesia, silica, &c.	2.94	3.78
Soluble salts	3.92	6.72
Organic matter, &c. (loss by ignition)	2.10	4.62
Alkalinity expressed in soda	.014	.056

The marked difference in amount of total solids per gallon, and in the amount of soluble salts and degree of alkalinity, shows that a decided change has been effected in the water by the time it reaches McNeill Street. In using it in the boilers there, it is only after a considerable time, and in corners of the boiler that a crust is formed. The ingredients of the water seem in general to re-act naturally, and in regular working only a deposit of loose mud collects, which is blown out of the boilers each morning.

The composition of the crust, as analyzed by Dr. Wallace, is given below, but a full analysis of the water would be required in order to show what re-actions take place in working:

Carbonate of lime	64.98
Sulphate of lime	9.33
Magnesia	6.93
Combined water	3 15
Chloride of sodium	.23
Oxide of iron	1.36
Phosphate of lime and alumina	3..2
Silica	6.60
Organic matter	1.60
Moisture at 212° F.	2.10
	100.00

The use of soda-ash as a preventive of the formation of incrustations in boilers working with calcareous waters is so rational and simple that it has, from a comparatively early date, commended itself to chemists, and has been, by them, repeatedly proposed to engineers. The material itself possesses for the majority of cases, where such is called for, all that it is requisite an anti incrustator should possess, while, if used with average care and intelligence, it is not capable of acting destructively. Its application is simple, as it may be added in solution either periodically—as in the case of the boilers quoted, or, better, regularly and steadily in fixed proportion to the quantity of water fed into the boiler, after the manner proposed by Mr. James Napier in the paper already alluded to. There is also no reason why it should not be added in proper proportion to the water in the feed tank or cistern, instead of being put into the boiler. In this way, as the re-action between soda ash and sulphate of lime does not require a high temperature or pressure, the precipitated carbonate of lime could be arrested, and comparatively pure water fed into the boilers, frequent blowing-off being also rendered unnecessary.

With waters containing only a little sulphate, and chiefly carbonate of lime, it would be necessary, however, to introduce the soda salt into the boiler, as a temperature of 100° Cent. (212° Fah.) is requisite for the decomposition of the bicarbonate of lime.

A short examination of the various remedies against incrustation which have been proposed, will not be uninteresting, although it results in the conviction that most of them are unsatisfactory.

Oxalate of soda and tannate of soda were proposed by Dr. Rogers in America, in order to form, by decomposition of the lime salts, insoluble oxalates and tannates; but these would seem to increase the amount of solid matter precipitated, and, although proposed some years ago, to have been little used.

Lime and zinc have been used with some degree of success, but their action is confined to combining with the carbonic acid of the bicarbonate of lime. On sulphate of lime they have no action.

The object in view in the proposed use of starchy and gelatinous matters has been to prevent scale forming, by enveloping the precipitated or crystallized solids with gelatinous covering, and so to delay their settling by diminishing their weight. But M. Bidard's observations on the effects of the presence of organic matters (especially in so-called "anti incrustators," or compounds for preventing incrustation) at once sweeps the field of all remedies of an organic composition, proving them to be injurious by doing the very thing which they are supposed to prevent.

Still, however useful as a precaution where the admission of extraneous organic matter can be prevented, this as a system of preventing incrustations manifestly fails where the water contains organic matter in solution.

Sal-ammonia, proposed by Ritterbrandt, and hydrochloric acid, are both open to the objections that their preventive action is only partial, and that they have the power of seriously injuring the boilers and connections.

Crude pyroligneous acid has been suggested for action upon carbonates alone, while Petroleum has been extensively used in the United States with a measure of success, not only in preventing incrustations, but also in removing those already formed. Its action has not as yet been investigated, as far as I am aware, but it is probable that its effects are due to decomposition of hydrocarbons. This seems to be borne out by a report by the Chief Engineer to the Steam Boiler and Inspection Co. of Hartford, U. S., who states that "petroleum works better where sulphate of lime predominates than in waters impregnated with carbonate of lime. We would not," he says, "advise it in connection with this latter." This simple fact renders it useless in the majority of boilers using fresh water in this country.

Soap acts upon both carbonate and sulphate of lime, but the quantity appears to be increased by the formation of lime soap, and thus the boiler is made filthy; a corrosive crust is sometimes formed, and priming and other evils also result from its use.

Recently a substance called "Burfitt's Composition" has been patented for the prevention of boiler incrustation, but it has been found to consist essentially of organic matters, and moreover has rather

increased than prevented incrustation where it has been used.

Two other methods of prevention have also been devised, and seem to be founded upon the fact which Professor Mills imforms me was first observed by J. Y. Buchanan, that barium chloride decomposes sulphates and liberates the carbonic acid in water.

One of these, called "De-Haen's process," which consists in the use of barium chloride and milk of lime, is now extensively used in Austria and in Krupp's Works in Prussia. A recently published statement of the comparative cost of working on this system, and with water containing gypsum, without an added re-agent, shows that to purify 33 cubic meters of water when containing five parts gypsum in 100,000, the cost is 6d., and when containing thirty parts gypsum the cost is 3s. Practical working with this process for twelve months with one or two boilers (it does not appear very clearly whether one or two), showed an increase of expenditure amounting to 500 florins, against which was to be placed the saving in fuel resulting from absence of incrustation, and reduced repairs from the same cause, the value of these, however, not being stated.

The analysis of deposits which have been found to accumulate in the steam pipes, &c., where these processes have been used, impress us with the idea that these methods are, however, open to some serious objections in practical working on account of the formation of salts of barium.

There is, however, one system of working which has yet to stand its trial, but which it is perhaps not extravagant to consider as inseparably connected with the advancement of engineering science and appliances upon which alone it depends. That is the working of land boilers in connection with surface condensers, and so supplying them with pure water. No incrustation is possible with this method, and its theoretical advantages in point of economy seem to justify the belief that its present limited adoption will prove merely the precursor to its more general introduction. It becomes of all the more importance in view of the extended use of sectional or water tube boilers, because with these, on account of small water spaces, no mere preventive measures against the formation of incrustation suffice. Solid matters ought to be excluded from all such boilers.

2. It is necessary, in connection with incrustation, to consider marine boilers working with sea-water, because although modern systems of marine engine practice with compound engines and surface condensers, wherever these have been adopted, have banished incrustations; yet these systems have not yet been universally adopted, and there is even a disposition with some to return to the régime under which incrustation held sway. The evil effects of incrustation make themselves felt with multiplied force in marine boilers, because of the great rapidity with which the crusts form, in consequence of the large quantity of solids contained in the water. I am informed by Mr. Tookey, of the Royal School of Mines, that British Channel water contains 2467 grains, and North Sea water 2408 grains in the gallon; and it has been shown by Mr. James R. Napier, F. R. S., that sulphate of lime begins to deposit before one half of the water is evaporated.

In addition to this rapidity of formation of crust, the space at command for storage of fuel is limited. Large quantities of chemical re-agents cannot for a similar reason be carried; and because of the confined space in which boilers and men working at them are placed on board ship, the results of an accident to, or the destruction of, the boilers are serious; while great difficulty is also experienced in getting repairs effected in foreign ports generally. All these considerations render it of the greatest importance that marine boilers should be freed from incrustation. Besides these, there are reasons connected with the formation of sea water scale, which render its presence in boilers undesirable.

Considered from a chemical point of view, the problem of preventing incrustation in these boilers, appears to be similar to that experienced with land boilers, inasmuch as the substances composing the crusts are similar in both cases, although differing in their proportions in the formations. No doubt, as we have seen, soda ash is the best chemical preventive where that substance has to be used in ordinary circumstances,

but the comparatively enormous quantity of solid matters present in sea-water causes the use of soda ash to be attended with so many inconveniences, and so much expense as to render it here practically useless. In these circumstances it has to be combined with blowing off from the boiler in order to get rid of the solids, it being necessary, as Mr. J. R. Napier showed, to blow off $\frac{1}{4}$th of the feed water, and neutralize the sulphate of lime in $\frac{4}{5}$ths with soda. The loss of heat from this blowing off is considerable, and it is combined with the cost of the large quantity of soda required for neutralizing. Yet this process is not worse than the ordinary mechanical one of discharging the saturated, or what is supposed to be the saturated, water from the boilers which has been most generally restored to. In this case the indications of the salinometer are depended upon, and fully $\frac{1}{4}$ths of the feed water have to be discharged, little of its heat being utilized. With regard to this Mr. Jas. R. Napier has said of the example of a vessel whose boilers worked at a temperature of 270° that "a quantity of fuel equal to 15½ per cent. of that which produces evaporation is consumed by the ordinary blowing-off method in order to prevent crust, and this amount increases with the temperature."

The salinometer might prove, and perhaps has often proved, a fallacious test, for if it were applied after a large quantity of the solids had been precipitated from the water, it would deceive the engineer by showing a less density than had existed previously, and thus mislead him as to the state of the boilers and of the water. In result it has been always necessary to chip and hammer away scale from the interior of marine boilers worked with sea water to an extent not advantageous to them.

Undoubtedly the most sensible and efficacious method of preventing incrustation in these boilers is to work with fresh water. This has been rendered possible in many instances by the introduction of the Surface Condenser into practical working, and no doubt the desire to avoid the evils of Incrustation has operated in bringing about the introduction of that system which is at present the most general in marine engineering. On the other hand, however, the commencement of this era in engineering practice has been the introduction of engineers to all the evils and difficulties of Corrosion.

Before dismissing the subject of Incrustation, I wish to direct attention to the following analyses and notices of the decompositions which take place in sea water in boilers during the formation of crusts, as these throw considerable light on the subject of Corrosion. The following analysis of Black Sea water was given me by the late Prof. Penny. I quote it only as showing the various ingredients contained in sea water, as I have no means of ascertaining its accuracy now—

Black Sea water, sp. gra.....	1.01365
Chloride of Sodium.....	14.02
Chloride of Potassium.....	.19
Chloride of Magnesium	1.310
Bromide of Magnesium	.005
Sulphate of Lime.....	.105
Sulphate of Magnesia.....	1.470
Carbonate of Lime.....	.365
Carbonate of Magnesia.....	.209
Total salts in parts per 1000..	17.674

The following analyses were made by Dr. Wallace, Nos. 1, 2, 4 and 5, for a paper of his on Boiler Incrustation, published some years ago; the others kindly undertaken for me along with other investigations inserted in this paper.

No. 6 differs from the rest in being merely a deposit. I have arranged them in tabular form, in order to show as far as posssible their relation to one another as having been formed at different pressures of steam—

(*See Table on following page.*)

Dr. Wallace, in his paper quoted above, remarks: "These crusts differ from the insoluble matter obtained by simply evaporating sea water in open vessels, for that contains nearly four times as much carbonate of lime as carbonate of magnesia, while the crusts contain a large quantity of magnesia, and little or no carbonate of lime. The decomposition of soluble magnesian salts by carbonate of lime under the influence of a liquid boiling at a high temperature (say 270°) is exceedingly interesting. Sulphate of magnesia and carbonate of lime boiled with water under ordinary circumstances, do not re-act upon each other in the slightest degree; but it is

ANALYSES OF BOILER CRUSTS AND DEPOSITS.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	No. 7.
Sulphate of Lime	33.95	66.88	69.77	74.21	72.85	57.34	76.83
Magnesia	40.05	18.96	15.75	14.95	13.18	1.94	1.81
Carbonate of Lime	—	—	3.44	—	34	—	—
Common Salt	traces	traces	.99	2.04	2.16	1.72	2.24
Phosphate of Lime, Alumina, and Oxide of Iron	1.33	.50	1.14	1.34	2.40	—	—
	—	—	.22	—	—	27.04	13.76
Silica	traces	traces	.16	.57	.80	7.60	2.24
Water with traces of Carbonic Acid	24.67	11.66	8 25	6.89	8.27	4.30	3.78
Total	100.00	100.00	99.72	100.00	100.00	99.94	100.66

No. 1 is from the Cunard steamer Asia, probably worked at about four or five lbs. per square inch pressure of steam.
No. 2 is from the King Orry, worked at about five to ten lbs. pressure.
No. 3 is from the Propontis, worked at about ten lbs. pressure. Old boilers.
No 4 is from the Cosmopolitan, worked at about ten to fifteen lbs. pressure.
No. 5 is from the Source unknown, worked at about (?) fifteen to twenty lbs. pressure.
No. 6 is from the Propontis, worked at about 150 lbs. pressure. Deposit before sea water was fed into Boilers.
No. 7 is from the Propontis, worked at about 150 lbs. pressure. Crust after sea water was fed into Boilers.

evident that the result is brought about under pressure. The re-action with oxide of manganese, which is isomorphous with magnesia, is exactly similar, and is taken advantage of in the recovery of the manganese used in the preparation of chlorine, as practiced at the St. Rollox Chemical Works.

"Again, the condition in which the magnesia occurs is peculiar. We should expect a basic carbonate, but I find little more than a trace of carbonic acid in any of the crusts. (In No. 1 it was .28.) The magnesia exists essentially as the hydrate. The sulphate of lime appears to occur as the hydrate described by the late Professor Johnson, as having been found by him in a distinctly crystallized condition in a high-pressure steam boiler, its composition being represented by the formula $2(CaOSO_3) + HO$."*

* Under the head Di-hydrated Sulphate of Lime, Gemellin says: This compound was deposited from the water in a boiler which was working under a pressure of two atmospheres; it formed a grayish granular mass of specific gravity, 2.757 appearing under the microscope in the form of small transparent prisms colored with carbonaceous matter.

		Calculation	Johnson.
2 Ca O_2SO_3	136.	93.79	93.272
HO	9.	6.21	6.435
Carbonic matter	—	—	0.293
Totals	145	100.00	100.00

Recently these results have been verified by the independent investigations of Dr. Ferd. Fischer who has proved from a number of analyses that various decompositions of the salts contained in waters take place under the influence of elevated temperature and pressure. Fischer quotes various authorities to show that gypsum gives off nearly half its water of crystallization at temperatures up to 100° (C.), and further proportions at higher temperatures, so that its solubility is considerably diminished. Above 140° it becomes totally insoluble in sea water, and at a lower temperature in fresh water, and hence is deposited as an anhydride. It is more easily soluble in water containing sodium or magnesium chloride in solution than in pure water. The effect of pressure on its solubility and that of other salts is shown by the following table of analyses of water from boilers:

(*See Table on following page.*)

He shows that only a portion of the calcium sulphate in boiler incrustations contains water of crystallization. In boilers which have been submitted to a very high pressure it occurs anhydrous. Magnesia exists as hydrate, the magnesium chloride giving up its hydrochloric acid under the influence of heat. The magnesium carbonate is decomposed at a

One liter of Water contains—	At 3 atmos.	At 1.5 atmos.	Taken with mud when blowing off Boiler.
$CaSO_4$ (Ca O,SO_3)	0.885 gram.	1.136 gram.	3.028 gram.
$CaCl_2$ (Ca Cl)	1.008 "	—	—
$MgCl_2$ (Mg Cl)	3.479 "	0.189 "	0.769 "
Na_2SO_4 (Na O,SO_3)	—	0.104 "	5.161 "
Na Cl	4.743 "	0.478 "	9.582 "
Residue found on Evaporation	7.210 "	—	18.864 "

temperature little above 100°, and magnesium sulphate undergoes mutual decomposition with calcium carbonate, the carbonic acid escaping. From a number of analyses given it is noticeable that the higher the pressure, and consequently the higher the temperature, up to 3 atmos., the larger the quantity of $2CaSO_4 + H_2O$, in comparison with the $CaCO_3$. But, contrary to the opinions of many, Fischer holds that carbonate of lime suffices of itself, or probably also aided by silica, to form a hard crust.

It appears from analyses of marine crusts to be probable also that the quantity of Na Cl in the crusts increases with increase of the steam pressure under which these have been formed.

THE EARTHWORK QUESTION.

By J. W. DAVIS, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

In a paper entitled "The Calculation of Earthwork," which was published in the October number of this Magazine, the author advocates a very novel way of calculating earthwork. Having a series of volumes of equal length to estimate, he constructs a new series of volumes, with same length and cross-sectional areas, but with unity for breadth. The longitudinal section of these volumes has consequently the same number of units of area that these volumes have units of solidity; and the altitudes of the cross-sectional areas have the same number of units of length that these sections have units of area. To this longitudinal section the author applies Simpson's Rule of integration for an area bounded by a curve; and, since the result also expresses the contents of the volumes of a unit breadth, he accepts it for the original earthwork volumes.

Now, we admit that Simpson's Rule applies very precisely to this longitudinal section, as it is constructed. We must consequently admit that it applies with the same precision to the constructed series of volumes. But we do not admit by any means that the new volumes are equal to the original earthwork volumes. Because these volumes have the same length and end areas, we do not see why the remaining boundary can be *assumed* such that Simpson's Rule, or any other, can be applied. Yet the author uses the language: "Since the area of the base of this [☞] equivalent [☞] solid," etc., without citing or creating authority for the statement. In a previous paragraph he shows that the content of an earthwork volume, by approximating end areas, is represented in the constructed volume with a straight line connecting termini of ordinates, as in Fig. 1 of his article; and that the content, by prismoidal formula, is represented with a curve, connecting ordinates, still nearer the axis; while Simpson's curve is beyond both. But in neither former instance is it shown that the constructed volume is not equal to the original, while in the last case it is merely assumed that the volume bounded by the outer curve is equivalent to the original. That is: Of three boundaries the author accepts the one he pleases, and proceeds to make

the statement that the prismoidal formula is less correct than the mere approximation of end areas, because its result varies more from that of the assumption than does the latter.

The author is also led into the following great error: "When the sections are equi-distant, the solidity of the excavation should be found by the prismoidal formula [ ] using measured middle area [ ]; in other words, by the application of Simpson's Rule, substituting the cross sectional areas for the lengths of the ordinates." Suppose we have three cross-sections equi-distant: to apply the prismoidal formula with these, as directed above, would produce enormous error, especially if the mid-section were near zero, or were much larger than the mean of end-areas, as is evident to every careful geometer.

We understand the author's general idea, and it is theoretically good; but we fear he has been carried away by it. He conceives that if the cross-section of a cutting or embankment be gradually moved from one end to the other, and the area at every point be represented by an ordinate, the termini of these ordinates lie on a curve, to the area bounded by which Simpson's Rule applies. This is very true; but, to make Simpson's Rule any where near correct, the sections must be taken at five or ten feet apart, whereas to use it with the field-notes of the present system, by which the work is carefully staked out so that it shall be contained by straight-lined volumes, or nearly such, is extremely wrong. But, if the cross-sections be taken five or ten feet apart, any reasonable method of approximation that ever was invented, is nearly correct, so the choice here would only be the simplest. In its best application, with close cross-sections, this plan should only be used where we have one gentle and continuous curve; but, to apply it to such an example as the one proposed above of their equidistant cross-sections, of which the middle is excessively large or small, and attempt to approximate the sharp curve, even if it were a curve, with Simpson's parabolas, between cross-sections fifty or one-hundred feet apart, is certainly a ridiculous use of Simpson's Formula: and a method which in general is not safe.

We advise the author to cling to the prismoidal rule until it can be *demonstrated* by himself or some one else that this venerable formula is false for earth-work volumes.

HYDRAULIC FORGING.*

By Mr. J. O. BUTLER, C.E., M.E.

From "Engineering."

The forging and shaping of malleable iron has been practiced from the earliest period. Long before the Christian era, it is recorded that there were "cunning workers in iron," rude in shape at first, no doubt, and with appliances of the simplest character. As time rolled on, however, and civilisation advanced, and men's wants multiplied, refinements in the mode of working produced superior "handicrafters;" and approaching our own times, we have sufficient evidence of this in our armories, museums, and arsenals, where are found in all parts of the world, magnificent specimens of workings in iron, chiefly however, for warlike purposes.

Coming nearer to our own times, and we may say to the commencement of the present century, or even so recently as forty years ago, the manipulation of malleable iron, when in an incandescent state, into shapes of almost endless variety, has been greatly stimulated by the ever-varying requirements of the practical engineer. The introduction of steam into our mercantile marine, and into our navy, as well as the locomotive engine, which was the natural offspring of our railways, has called forth the inventive genius of our workers in iron in a most remarkable manner, and perhaps no

* Paper read before the Leeds Meeting of the Iron and Steel Institute.

machine has brought out the talents of our working smiths more than the locomotive engine.

The writer well remembers the time when smithwork, even of a comparatively simple kind, was difficult to be executed, and it was often the case that the designer and the draughtsman had first to chalk out the piece of work he wished to be done in a particular fashion, on the fore plate of his smith's hearth, and ask his workman, "Can it be done like this?" When he would often reply, "No, maister, it cannot be done."

We must not, however, omit here to mention that the rapid strides made in the operations to which allusion has been made, are due almost entirely to the steam hammer, that wonderful production of the brain of a Nasmyth, which, about thirty-five years ago, revolutionised the operations in our forges and smiths' shops. In the present day, masses of iron of immense magnitude are being manipulated with ease that once was thought impossible—witness our "Woolwich Infants" and other kindred pieces, such as armor plates, and the like, which have first to go under the steam hammer.

The pressing of iron into a mould, or matrix, to give shape to various articles by the aid of the screw press, has also been practiced for many years; the steam hammer has likewise been brought into requisition for the same purpose, but to a limited extent only. Reciprocating blows from a steam hammer, it is found, do not produce or accomplish satisfactorily the kind of pressure necessary for forcing the atoms or molecules of iron, in an incandescent state, into all the interstices of a mould, where intricacy and accuracy are desired. This, however, can be done effectually by the inexorable thrust of a hydraulic or hydrostatic "squeeze." And this leads us to the subject of the paper now before you.

We believe that Mr. Haswell, of Vienna, was the first to bring into practical and useful operation the "squeezing" of malleable iron at a welding heat into shapes and uses, as they are technically called, previous to their being manipulated by the smith and fitter. Some years before Mr. Haswell's patent of the machine, or tool, now under consideration was designed, hydraulic power had been made use of for forging or pressing malleable iron, both with and without the aid of an accumulator; but it is to Mr. Haswell that we are indebted for the improvements which make the hydraulic press a tool of general use. The drawings or photographs against the wall illustrate the machine or tool that he has produced for that purpose. It will be perceived that it is simply the adaptation of the hydraulic press, on the principle of Bramah, with an arrangement peculiar to Haswell, whereby a "squeeze" can be given, either reciprocating, or in one continuous thrust, until the piece operated upon acquires the desired shape. The pieces on the table are samples of what are produced: No. 1 is a sector of a 12-spoked wrought-iron locomotive wheel, showing three spokes with their portion of rim and boss pressed out of the solid slab. No. 2, locomotive cross head ditto. No, 3, ditto, (double), ditto, ditto. No. 4, outside crank with its pin, ditto. No. 5, piston rod socket, ditto. No. 6, locomotive axle-box, ditto.

Description of the Machine.—The pumps, as will be seen on the drawings or photos, are worked by a horizontal direct-acting steam cylinder, of large size, the working of which is directed by an automatical arrangement, so that it is perfectly and instantaneously under the control of the driver. The press consists mainly of two vertical cylinders of different sizes. These cylinders are fixed to a large cast-iron frame of cruciform shape, and may be termed the pressing and lifting cylinders respectively, the one below and the other above. The frame is supported on four malleable iron columns, or pillars, which are firmly secured to a bedplate of a corresponding shape to the frame above. The two pistons or rams have cross heads fitted on their outer ends, connected at their extremities by two strong malleable iron side rods, so that the pistons or rams work simultaneously. These rods, passing through grooves formed in the upper frame, and the intermediate steadying frame, act likewise as guides to prevent the rams turning. The hammer, or upper mould, or matrix, is, of course, fixed or fitted to the lower end of the pressing ram, and the bottom anvil, or

mould, or matrix, is firmly bedded to the bottom bedplate.

The main steam cylinder is fixed horizontally on a strong bedplate, the piston rod passing through packed stuffing boxes at both ends, to which are attached, in the same line and plane, the rams of the respective hydraulic pumps. A small steam cylinder is placed by the side of the main steam cylinder to work its slide valve. Attached to the valve rod of this small steam cylinder by a lever is a tappet rod, which is worked automatically by a projecting arm attached to the cross head of the ram of the pump, this acting upon the slide valve of the small cylinder, which in its turn actuates the slide valve of the main cylinder which works the pumps. The discharge branches of the two pumps are connected by pipes with the passages and chambers. The inlet and discharge regulating valves are worked by strong levers, having rods attached to their free ends, and directly connected with the piston rods of the two small auxiliary steam cylinders. The slide valves of these auxiliary cylinders are respectively worked and controlled by means of the hand levers. The small cylinders, and which we may term cushion cylinders, are charged with oil, and have perforated pistons; the rods of which are also connected to the regulating valve levers before named. These cylinders act as cataracts or buffers to relieve the sudden "chuck" incident to the work put upon the levers. There is also a loaded safety valve, not shown on the drawings, placed in a convenient position on the pressure pipe for relief in case of need.

The press may be worked at any desired pressure, regulated by the boiler steam pressure, and either a light or heavy blow, or squeeze, can be given to suit the work in hand. The velocity or number of strokes per minute depends upon the efficient action and rapidity with which the two auxiliary cylinders can be worked, as these regulate the inlet and escape valves, the driver having merely to handle the levers of the slide valves. It will be perceived that no expensive foundations are required, as both the engine and press are self-contained.

ENGINERING AS A PROFESSION.*

By Prof. REYNOLDS, of Owens College.

From "Iron."

Next to the desire which man has to possess good things for himself is the desire to hand down these good things to posterity. The certainty that all things were coming to an end in fifty years would send us about our work with strange feelings—feelings such as are to some extent raised by the vague certainty that coal will not last for ever. Most of us lay the flattering unction to our souls that it is not simply for ourselves that we are working, but that in some respects we shall earn the gratitude of future ages. History, however, teaches us that societies, as a rule, only flourish for a limited period; that sooner or later the seeds of decay are sown, and the descent having once begun downward progress is rapid. Continual watchfulness is the only safeguard, and there is nothing which so much conduces to the neglect of this as a period of great prosperity. It is in such a time that the evil seeds are allowed to take root and get to such a head that when perceived they are too strong to be eradicated.

There is no class of men with greater responsibilities than engineers—not only in money, although they are supposed to be answerable for some of this. It is in human life that the engineer has his highest trust. Mankind has placed itself unreservedly in his hands. On the skill and conscientiousness of the engineer the lives of millions daily depend; and this is not only as travellers—on the construction and working of railways, bridges and steamboats; but in our

* An address delivered by Prof. Reynolds, as president of the Manchester Scientific and Mechanical Society.

houses and streets we are undermined and surrounded with boilers, the failure of which would be certain death to those who probably do not even know of their existence. And many of our towns are overhung by reservoirs of water capable, as we know from sad experience, of sweeping away a large population at a single stroke. Nor is this all. It is not only in the stability of his completed works that the lives of his fellow-creatures depend on the engineer. He has to send men into dangerous places, to contend with the forces of nature, with nothing but his intelligence to assure him of their safety. For example, should the Channel Tunnel ever be completed, large bodies of workmen will have to go for five or ten miles under the sea. They will have to attack a citadel day by day for ten years, with no line of retreat except that which is covered throughout its entire length by the fire of the enemy. That men will be found to go, no Englishman doubts. The pluck which has stood us in such good stead against the forces of man is one of our chief sources of success in our contest with nature.

It cannot be said of the engineer, as it must be of the lawyer and doctor, that if we could do without him it would be well for us; that, great as is the good which he does, the necessity for his presence is a misfortune. Individually, engineers are as good as any other class of men. I do not say better, but as good; and, during the comparatively short period of our importance, we have brought to the front as many men of whom the nation, or I may say the world, is proud as any other calling. But it is not as a collection of individuals that I am speaking of engineers. It is as a body. We are sometimes spoken of as a profession. We have responsible work, and men capable of doing it—two important qualifications for a profession. But I think the term "profession" implies something more than this. I think it implies a certain coherence and internal organisation; and this, I fear, we do not altogether possess.

There can be no doubt that a change is now rapidly, if gradually, coming over the way in which engineers have to work. The art of construction, which for more than a century has been in a state of revolution, is now fast settling down into the application of well-tried principles and rules, however extensive these may be. By far the greater part of the work which comes to the engineer is work which is capable of being performed by strict rules and precedent, just as the work which comes before the lawyer or doctor. Ninety-nine per cent. of the work is similar to what has been done before, and in such cases the only course an engineer can have for falling back on his inventive power or originality is one that he should be ashamed of, namely, his ignorance. There was a time it is true, when this was not the case—not long ago, but quite recently. Up to the end of the last century the art of construction was regular and steady-going enough; and the rules by which engineers or architects worked were well defined. Then came the revolution and the grand advance—the introduction of steam and iron. During this revolution new rules had to be discovered. Then was the call for originality, which brought those to the front with whose names we are most familiar as engineers; these were not, as a rule, either trained as engineers or acquainted with the rules by which engineers of that time were guided. Brindley was a carpenter, Smeaton a mathematical instrument maker, as also was Watt, while Telford was a mason. It was by virtue of their original gifts that these men succeeded in making the advance. Ignorance of the then highest art of working in wood and stone was no great drawback to them, for they had to introduce new material with new objects. Now, however, the case is again different. The period of revolution is past. A new art has taken the place of the old, but it is again an art with its definite and distinct law and precedent. Originality is required to advance this art and improve the laws, but not to tide over our ignorance. Yet this is a duty it performs only too often.

The way to excellence in engineering now is, not promiscuous trial and happy guesswork, but by carefully studying what has already been done. Formerly the only education was that of actual experience. He had to discover everything for himself. Now, however, he must be guided by the experience of others, and he must expend earnest work

in gaining this experience and preparing himself to appreciate it. Formerly an ordinary school education was considered ample preparation for an engineer. He was then sent into the works to pick up what he could, and develop improvements by a process of unconscious observation. Of this plan the latter part at least was sound—namely, that of seeing large quantities of work before he himself was entrusted with the execution of it. In my opinion nothing can make up for the want of actual experience. No amount of learning about things gives a true idea of the things themselves. It is the main fault of Continental schools of engineering that a course in the works does not form part of their curriculum.

Those who, in spite of their early paucity of education, and there have been many, have, notwithstanding, acquired a fair working knowledge of what we may call the theory of their subject, have done it by undergoing the greatest hardships. They have spent years of weary work in acquiring what would have been a comparatively short and pleasant task if systematically attempted with due means. I once heard an engineer, now of great eminence, when speaking of the comparative facilities now afforded, say, "With a great sum obtained I this freedom; but you, like St. Paul, are free born." I doubt not that there are others who have given up as hopeless the attempt to understand comparatively simple matters, because it was wrapped in forms into the mysteries of which they had not previously been initiated. One of the reasons why preliminary education was held of such little account has been the character of the only education to be obtained. These things, however, are now altered by the establishment of special schools and the extension of the old. All the more useful branches of science are now within the reach of the student of engineering, and in the forms most suitable for him. So that, as a step towards understanding the theory of machines, it is not now necessary for him to begin with the theory of astronomy or the doctrine of chances. The miserable form in which the only mathematics to be obtained was wrapped has compelled engineers to work out methods for themselves, and now that the demand for such knowledge has increased, we find that the first mathematicians in the land, have, so to speak, patronized our system. And as is natural, the extension of the practical use of mathematics infused new life into their study. Learnt as it now may be with a special view to the application of practical mechanics, a knowledge of mathematics and science is much more useful than it was. But this is by no means all. This would not be much were it not that the accumulated experience of engineering work has to a great extent been systematized and reduced to a form capable of mathematical treatment.

The case is, however, entirely altered. Whereas formerly the good, at all events, the immediate good to be reaped from the higher theoretical studies for those designed for the calling of engineers was doubtful, now there is every inducement for it. Nor has this fact been lost sight of by engineers. To their credit, it must be said, that they have come forward liberally to provide their successors with that education of which they have avowedly experienced the want. There are now some fifteen colleges and universities in the country where not only can a knowledge of all the useful sciences be obtained, but where the application of science and mathematics to the work of the engineer is made a special branch of study. The advisability of such a course of training is even now sometimes called in question. But I think this doubt can only apply, and is only meant to apply, to such a course of study as constituting the sole education of the student—as displacing the practical training. Looked at in this way, the doubt is just, for, as I have said, no amount of theoretical education can give that certainty and facility which only come from practice. As an adjunct to the practical training—as a preparation for it—there cannot, I think, be the least doubt. The field of engineering has become so vast, that it is impossible for any one to acquire anything like a complete acquaintance with it by practical observation. The actual work of which one can gain experience in the course of a few years is but small, even under the most favorable circumstances. And the only way to make use of such experience as a general training is to supplement it by reading, and thus to

use it for the purpose of illustrating the application of general laws and principles.

Even allowing the aid of books, the range of work which may fall to the lot of an engineer is far too large to be mastered by one mind, unless reduced to a system and to general laws. Thus all the multifarious forms of structures: buildings, bridges, pillars, wheels, roofs, &c., which, if each one is to be treated as a whole, must be numbered by thousands; if divided and considered in their component parts, are found to consist of seven or eight simple structures; and the laws which regulate the use of these may be treated separately. Or, again, endless as are the varieties of machines, when divided into their elementary parts these are not found to number more than 100. And so we might go on.

It is, then, clear what an immense advantage is to be gained by attacking this mass of knowledge in a systematic manner, such as that in which it comes before the student in his course through a college. This is, in truth, the only manner in which anything like a complete mastery can be obtained. To attempt it by private study is to work at a great disadvantage.

The exact course of preparation which is best for a student of engineering to pursue, although it should be varied according to circumstances, seems to be somewhat as follows: Assuming, as in other professions, the age at which he is supposed to commence his career to be about twenty-one or twenty-two. Having pursued a general course of education at school until he is sixteen or seventeen, he should then commence his special course. In this he must learn something of science and something of art; but his main object will be to learn how the one can be brought to bear on the other. Mathematics and the natural sciences must form an essential part of his study; but he must not expect to make himself completely master of either. To do this would occupy more than the whole time at his disposal. He must select those branches of those subjects which most directly relate to his future work, and leave the rest as he would leave a luxury. The making of this selection is very difficult; the temptation is always to attempt too much, and this ends only in confusion. It is but a comparatively small portion of these wide subjects that can be usefully brought to bear on engineering, and to these he must necessarily restrict himself. The methods of applying these sciences to engineering problems constitute a large subject, and one that it is necessary for him to study; and besides this, he will have to devote some of his time to acquiring sufficient knowledge of the things to be done by engineers on which to study the application of his science. And then there are yet those manual operations which are essential to bring his knowledge to a practical issue, and in which a long course of training is necessary to acquire the requisite skill, such as mechanical drawing and the use of measuring and surveying instruments, the want of facility in the use of which would prevent for a long time the student from making practical use of his knowledge.

To acquire a useful knowledge in these various branches of study will require three, or at least two years. The student will then proceed with his practical training, which should include as great a range of work as possible. In this he will find the knowledge he has acquired of very great help; he will recognize much that he sees, and be able to judge of the most important things to which to direct his attention. After such preparation he will learn more in one year spent in the workshop or on the works than in three without it, so that by the time he has completed his training he will have as much practical knowledge as if he had spent his whole time in the workshops.

Of course it would be little short of affectation to pretend that, surrounded as we are with mechanical results, one cannot learn to produce the results with which he is familiar unless he is first able to deduce them from elementary principles. This would be equivalent to asserting that an English child could not speak English until he had mastered the rules of grammar. But to teach a language without the aid of grammar is not only a waste of labor, but a sure means of producing an imperfect result, and this is equivalent to teaching engineering without science. Such is the hold which the study of natural science has

taken on all classes, and such are the facilities for those in the lower ranks to rise, that it seems to be quite certain that if those who have the best opportunity of qualifying themselves as engineers neglect to do so in the highest manner, they will find their places filled by those who, while rising from below, have made better use of their opportunities.

To the body of engineers, as a whole, belong the duty of prescribing the course of study which shall entitle any one to rank as one of them. It is not, however, too much to say that at present nothing of the kind is attempted in England; and although various ways in which it might be done have been under discussion, apparently no conclusions have been arrived at. It is done in other countries. And the fact that it is done by other professions—not only the older ones, such as law and physic, but by the most modern, such as that of actuaries—is a sufficient proof that it may be done by engineers.

What are the difficulties? Why could not the representative bodies of engineers constitute themselves into examining bodies like the College of Surgeons or Physicians, and only admit such fresh members to the various grades within their ranks as should satisfy them as to the sufficiency of their education? Such a plan, if effectually carried out, would be a great advantage both to the outside public and to the engineers themselves. It would give a young engineer a definite object for which to work, as well as an opportunity of at once proving his capacity; while, on the other hand, it would purify the ranks of engineers, and block out the crowd who now press into the calling simply because they have already failed, or feel unable to pass the definite tests required for other callings.

Of the pupils who enter the engineers' workshops, or otherwise commence the study of engineering, it is only too apparent what a large proportion are of this class, and I believe that they choose engineering solely in the hope of missing all examination. There can be no greater fallacy. Engineering is one lifelong examination. There is no slurring over mistakes. Either your work fits or it does not; your bridge stands or it falls; your machine goes or it stands still. By a mistake an engineer loses something besides a case or a patient. If there is a reason why those entering the profession need not be examined, it is that their whole career will be a practical examination. This eventually turns out those who are inefficient, but unfortunately it does not help to bring the efficient members to the front at starting. This is the real object for which a test is wanted, to give the hopeful students a definite and not far distant object to work for; and to offer an inducement to them to undertake the ardous task of a high-class training, which, although invaluable to them when they come to be engaged on important works, and not then to be attained, will have but little immediate effect in forwarding their chance of employment.

It may be said that there are already institutions which require some qualification for membership. Far be it from me to decry these institutions in any way. But what are the tests? I believe the only one required for membership of the Mechanical Engineers is the payment of a somewhat high annual subscription; whereas for the Institution of Civil Engineers, although the qualification is high for membership so as to render the standing very valuable, it is entirely a test of position, not of capacity, and hence is altogether beyond the reach of the young engineer; nor for the only other grade, that of associate, although this is conferred more freely, is there any examinational test. The only approaches towards such tests are the degrees and certificates in engineering science conferred by the various colleges and universities. These, however much they may be sought for the intrinsic value of the education they imply, are not as yet generally recognized amongst engineers.

The value of a degree in engineering from such a body as the Institution of Civil Engineers has been questioned, because it is said that they could not withhold the right to practice. No doubt in one sense this is true. But it may also be said of the representative bodies of any other profession. The right to practice can only be withheld by the State, and it is by their permission that the holders of degrees in law and medi-

cine are allowed to do so. That the State would reserve the right to practise as engineers in general to those holding degrees is hardly probable, but that it would do so in some respect is certain. In place of a degree the passing of a special examination is required for all civil service appointments. And there are many classes of work where if there were any qualification test this would be insisted upon. Wherever a man is responsible for the safety of his fellow creatures it is the duty of the State to step in and insist that the man shall be qualified for his post.

In the medical profession we find men of the greatest experience—those who are doing the hardest work—hold it a matter of honor and duty to come and give the students the benefit of their experience. The medical man looks back with pride and pleasure to his career as a student, and to the connection into which he was then brought with the greatest men of his time, to whose words he listened with the enthusiasm which is ever stirred in the breast of a recruit by the account of battles from the lips of a veteran. Nothing impresses on the mind so strongly the importance of unity as early association. In this direction I hope we shall soon be able to move; and that with united effort the time may not be far off when the engineering profession may be able to hold its own, not only in the importance of its work and the capacity of its individual members, but as a corporate body, discharging all the functions necessary for handing down its importance to posterity.

THE UTILIZATION OF SLAG.

By Mr. BASHLEY BRITTEN.

Transactions of the Iron and Steel Institute.

Few of the substances which occur as manufacturers' refuse or bye products have arrested more attention than the scoria or cinder formed in the smelting of iron ores. The vast quantity produced, and its liquid form, at once suggest the idea of extensive usefulness; and all that seems necessary at first sight is to collect it as it flows and forthwith cast it into a thousand articles of utility. Experience, however, has shown that this is attended with difficulty arising from its behaviour as it cools. When allowed to solidify slowly the particles of slag arrange themselves in a crystallised form, and the substance becomes tough and presents the well-known grey stone appearance; but if chilled by contact with a mould, or any surface causing rapid congelation, it becomes vitreous and brittle, and of a black or dark brown color, so that in the same piece two wholly dissimilar structures may often be seen. Unequal tensions are thus occasioned, causing the substance to crack and break unless prevented by such slow and careful annealing as involves considerable trouble and expense. The purposes to which at various times it has been proposed to apply this marerial are strangely various, and among others are those of casting it, in imitation of stone, directly into paving blocks, slabs, and pipes; of coloring and making it into decorative tiles and columns to resemble marble; of shaping it into architectural ornaments of black obsidian; of converting it into bricks; blowing it into a white wool; pulverising it into cement; and also of reducing it in water to the state of sand, in which form it has been advertised as a valuable manure.

Most of these schemes have failed and been abandoned; still the building and paving materials now made from slag at Middlesborough seem of good quality, and it is to be hoped that their manufacture will prove remunerative; but the cost of carriage for such weighty and necessarily low-priced articles, would appear to be only too likely to confine them to a comparatively narrow local demand. The only use as yet found for any considerable quantity of slag is to break it up for road-making, and to

build rough walls and foundations where stone is scarce. By far the major part of what is produced is wholly valueless, and the iron smelter has not only to bear the cost of moving it out of his way, but frequently must purchase land on which it may be deposited. It will support no kind of vegetation, as must be well known by those who have seen the huge barren mounds which, in all the iron districts, disfigure the country for miles. In Yorkshire alone upwards of 4,000,000 tons of this ugly refuse have annually to be thrown away; while the total quantity made throughout the country probably exceeds four times as much as this. Any method, therefore, of checking this accumulation, even to a small extent, may fairly engage attention.

There is no novelty in the idea of employing slag as an ingredient of glass. It has long been so used in this country to some extent, but on a larger scale abroad. In the Forest of Dean persons are employed about the old charcoal iron cinder heaps to collect the more vitreous-looking pieces, which are supposed to be the only suitable portions, and these are put under stump heads and pulverized in water; the resulting sand is then dried and sent to the glass-makers of Bristol, who mix it with their other materials and melt them down together. This practice is, however, fast becoming obsolete, because it is found that the cost of preparation and transport, and the very intense heat required to forge slag which has once cooled, leaves no advantage from its employment instead of other materials. A few years ago the price paid for this slag sand delivered at Bristol was as much as 20*s.* per ton, and this in 1875 had diminished down to 12*s.*-6*d.*, which was said to be scarcely remunerative. For perfectly white glass, such as crystal, it is obvious that slag can be of no value at all, in consequence of the amount of iron it contains, which cannot be eliminated, and would produce a green or amber color. Still iron is present more or less in all glass. The analyses of specimens of the window glass of commerce exhibit as much as from one-half to one and a half per cent., it being possible to neutralize its effect to a considerable extent by decoloring materials. For all glass in which a tinge of color is either needed or is not detrimental—and this includes an extremely large proportion of all that is made—a little iron does no harm; it is, in fact, often purposely introduced as an important element, for it is capable of replacing other flux, and so lessening the amount of alkali which would otherwise be required. Some estimate of the probable intrinsic value of slag to the glassmaker may be seen from the following table, in which the ascertained composition of slag of an average description is placed side by side for comparison with that of common bottle glass :

TABLE No. 1.

	Composition of Iron Slag. (Welsh or South Staffordshire.) Per cent.	Composition of Bottle Glass. (Quantities variable.) Per cent.
Silica	40	45 to 60
Lime	35	18 to 28
Alumina	16	6 to 12
Magnesia	6	0 to 7
Alkali	1 to 2	2 to 7
Oxide of iron	½ to 2	2 to 6

A trace of sulphur is also found in slag associated with the lime, but this need not be regarded, as it is insignificant in quantity and readily passes away with heat. It will be observed that there is nothing in this waste of the blast furnace which is not represented in glass. It is also clear that it really contains less iron than is often required by the glass-maker. Some striking facts, therefore, arise, which may be pointed out. The glass-maker must buy all the above ingredients or their equivalents. They represent large quantities, and he has to pay for their conveyance to his premises, where they have to be carefully mixed and moved again, and finally put into his furnaces, there to be kept at a white heat for many hours till the highly refractory substances are dissolved and fused. The ironmaster, with a different object, is always melting down precisely the same materials; he cannot produce his iron till he has first converted the gangue of his ore into glass; but this he calls "cinder," and gets rid of it as best he may. The cinder is not readily recognized as glass because it looks like stone; but all glass will look like stone if cooled slowly from its liquid state. The cause of slag being so completely devitrified is that it is too rich in lime and too poor in silica. The glass maker, however, has

to buy this lime at five or six times the price of his silica or sand, while all his ingredients together cost him comparatively little more than the very large quantity of fuel he must burn, in order to bring them into the melted form in which the ironmaster throws all his glass away. The question, therefore, to be opened out is whether there may not, to a great extent at least, be a remedy for these practical anomalies. May not the manufacture of crude iron be so combined with that of glass, that the latter may be produced so cheap as to render it available for many additional uses, possibly more extensive than those to which it is at present applied?

Glass making now fairly ranks among the largest industries of the country, and commands a wide export trade. It is a material of great beauty as well as strength, and, being nearly proof against all chemical reactions, it is quite unrivaled for durability and cleanliness, and with those qualities it is capable of being manipulated or pressed and moulded into shape with greater facility than any other known substance. If then, as will be found probable, it can be produced in finished forms cheaper than any other material, not excluding even stone, slate, or common earthenware, it is difficult to foresee the extent of its application. At scarcely any extra expense it may be made of almost any color, or absolutely black like jet, in which latter form it might, for many purposes, serve better than the finest marble. As one example of usefulness, its suitability for roofing may be mentioned. It may be either opaque or clear to admit light. If opaque, it would be less transparent to heat than slate, and as it could, without skilled labor, be rolled into the desired forms, either plain or highly ornamental, it is likely that they might largely take the place of slates, by being sold at a lower price, and yet at a handsome profit.

The constituents of slag are common to all kinds of green glass used either for bottles, or windows, or even mirror plates; therefore, by diluting them with the usual pure materials to a greater or less extent, the compound may be brought up to any standard short of the purity from color inconsistent with the iron, or sometimes manganese, residing in the slag. Perhaps it is scarcely to be expected that slag glass will ever be produced quite equal in color to the ordinary best window glass; still it would be premature to anticipate what may be done should it ever become worth while to adopt special means for rendering the slag purer. In regard to all other essentials, such as clearness, brilliancy, strength, plasticity in working, power of resisting acids, and the capability of being cut with a diamond, it may be made equal to any other.

The practical question has next to be considered of how far it is possible to combine the manufacture of glass and iron without in any way interfering with the necessary continuous operations of the blast furnace, for this, as a matter of course, is absolutely essential. Blast furnace works where pig iron only is made frequently stand in pairs, in isolated situations, with plenty of space around, on which glassworks may be erected on any scale; and in many instances they might be built close up to the sides of the furnaces, and extending laterally away from the pig bed. In that case the slag might be run directly into a glass furnace, on the well-known plan of Mr. Siemens, for continuous founding and working. Where there is insufficient room for this, the glassworks might be at some distance, and the slag could be collected and conveyed to them in a state of fusion in large covered iron ladles on wheels, similar to those used in some Bessemer steel-works, where the molten iron is carred upwards of a mile to be poured into the converters. Various other plans will suggest themselves to suit the exigencies of existing works.

The foregoing observations are founded on the results of a long series of experiments extending over the greater part of the last three years, in which the author has endeavored to test, in every way open to him, the soundness of his conclusions before submitting them to criticism. A few specimens of the glass produced from slag are on the table for inspection, and they are not to be regarded as perfect, but only as indications of what may be expected from developed means. They are, in fact, only experimental results in the making of a few hundredweights at a time. The glass can, of coure, lay no claim to high quality in point of color; still this is its

only inferiority, and no doubt it may be improved in this respect. But it may be observed that it is as nearly as possible the same tint as that purposely made to suit the requirements of horticulture, as evidenced in the great conservatories at Kew. It is also but little, if any, darker than a great deal of what is now used in the form of rough plate for sky-lights. It is well known that a tinge of green is of great utility in checking the heat rays and softening the glare of white light. Although slag-glass cannot compete with that higher quality which is produced so largely in the neighborhood of Leeds, it certainly has the recommendation of great cheapness, for it is doubtful whether glass as good could be made for less than three or four times its cost when everything is taken into account. A very large proportion of it represents so much material and fuel which is now utterly wasted, and it only remains to point to the enormous quantity of colored glass now employed for various useful and ornamental articles, and then to ask, what further uses may be found for such material?

REMARKS ON THE CONTINUOUS TRUSS QUESTION.

By CHARLES BENDER, C. E.

Written for Van Nostrand's Magazine.

The late note on this question in the November number of Van Nostrand's Magazine, gives room for further corrections of errors therein committed:

1/ The theoretical quantity of web-material of a continuous bridge, under full uniform load for any number of spans, and for any proportion of spans, is greater than the corresponding quantity for single span girders.

In the October number of the Magazine, this proposition was proved mathematically, and generally as a correction of one of Mr. Merriman's errors.

He now admits it, but not without adding another mistake, easily proved. He says that by slightly reducing the lengths of the end spans of a continuous bridge the theoretical web-material can be made even lighter (!) than for a single span.

This is radically wrong, for under such construction the theoretical quantity *becomes heavier*, not lighter.

Suppose we have a bridge of three spans, total length = L. Divide it into three spans, x y and x, so that $x=\frac{L}{3}-d$, and $y=\frac{L}{3}+2d$.

We proved, and it is also admitted, that the *least* theoretical web-quantity, under full uniform load or under dead load, is equal to

Coefficient $\times\frac{x^2}{2}$, respectively. Coefficient $\times\frac{y^2}{2}$

(See Magazine, page 290.)

Hence we have, assuming at first a coefficient, constant for three spans:

$$\text{Web}=\frac{\text{Coefficient}}{2}\times\left\{2\left(\frac{L}{3}-d\right)^2+\left(\frac{L}{3}+2d\right)^2\right\}=\text{Coefficient}\times\left\{\frac{L^2}{3}+6d^2\right\}$$

This equation shows that the web-quantity is a minimum in case $d=o$, that is, if the three spans are equally long, and this minimum for three independent or single spans is:

$$\text{Coefficient}\times\frac{L^2}{3}$$

If, as suggested, the three spans x^2, y, x^2, are made continuous, the web-quantity for the center span will remain the same, namely, coefficient $\times\frac{y^2}{2}$, whereas the material for the outer spans becomes *greater*, hence the theoretical quantity of web-material for the arrangement proposed by Mr. Merriman will be *greater* than

$$\text{Coefficient}\times\left\{\frac{L^2}{3}+6d^2\right\}$$

while the minimum theoretical web-material would be

$$\text{Coefficient}\times\frac{L^2}{3},$$

the difference against continuous trusses being even *greater* than coefficient $\times 6d^2$.

Now, suppose the coefficient for the short spans to be C, less a small quantity

Z, and that of the middle spans to be $C+Z_1$, then we get:

$$\text{Web}=2\,(C-Z)\times\left(\frac{L}{3}-d\right)^2+(C+Z_1)\times\left(\frac{L}{3}+2\,d\right)^2$$

But Z and Z_1 are approximately $\frac{3\,d\,C}{L}$ and $\frac{6\,d\,C}{L}$. Hence the

$$\text{Web}=2\,C\left(1-\frac{3\,d}{L}\right)\left(\frac{L}{3}-d\right)^2+C\left(1+\frac{6\,d}{L}\right)\left(\frac{L}{3}+2\,d\right)^2$$

$$\text{Web}=C\left\{\frac{L^3}{3}+6\,d^2+\frac{6\,d}{L}(2\,L.d+3\,d^2)\right\}$$

which is even greater than the corresponding value under the supposition of a coefficient C, constant for the three spans x y and x.

These facts are illustrated by the figures in our examples on page 294 of this magazine.

2/ Our critic tries to evade the weight of the mathematical deduction of the proposition (page 299), that the deflections of continuous trusses form the teststone of the validity of theory.

Only this sentence needs comment:

"And even if this be granted, it must be proved, and not assumed, that the slightest variations of I and p, which scarcely influence the reactions, do not affect the far more delicate(!) deflections."

In answer it is said that none who is accustomed to interpret mathematical expressions should have made any such remark.

Our mathematical expression for the value of deflection was:

$$\text{E. I. } d=\frac{A}{6}\,(x_1^3-l^2\,x_1)-\frac{p}{24}\,(x_1^4-l^3\,x_1)$$

The reaction A is known to be directly proportional to p, so that, if p alters slightly, also A must alter slightly, and consequently also d must alter slightly and proportionally with p.

If I alters slightly, d must alter slightly though inversely. Hence, it is absolutely wrong to assert that slight alterations of p and I make great changes in the value of deflections. *They only cause proportionally small changes.* All this is very elementary indeed and the learned French engineers, before giving their confidence to the theory of continuous (plate) girders, compared the actual deflections with the theoretical or expected deflections which in *their* examples were found to agree within what they considered limits sufficiently close as to warrant the practical use of those formulæ for their (plate) girders.

On this point Professor Calcott Reilly, of the Imperial Indian College of London, in the discussion of Mr. Gaudard's paper "on the present knowledge as to strength and resistance of materials" (Proceedings Institution Civil Engineers, Vol. XXIX, page 85, year 1870) laid special stress.

3/ Mr. Merriman does not at all enter into discussion of the important question of depths of single spans and at continuous bridges. Nor does he pay attention to the correction due to the influence of the webs on the deflections and consequently reactions, strains and sections of continuous truss bridges. And yet the influence of the web thoroughly vitiates the theorem of the three moments such as derived from the common theory and if applied to deep trusses.

Hence also the reactions calculated therefrom, hence the moments over the middle piers and of course also Mr. Merriman's triangle, by itself only a curiosity, are entirely vitiated.

We have given the strongest possible arguments and proofs, why continuous bridges are deficient in their theory, and why they cannot be lighter, either theoretically or practically than properly proportioned single span trusses. We also have given careful estimates as to the theoretical and the practical quantities of single spans and of continuous bridges of two and three spans which fully illustrated the truth of our theses.

Now it remains for Mr. Merriman to soon go to work and prepare full designs for a bridge-crossing with continuous and with single spans.

By doing what we proposed for three spans of a total length of 1,000 feet, he can *calculate* the *actual* quantities within one-half of one per cent. and thus he can very readily *try* the theory embraced.

Hic Rhodus, hic salta!

4/ The fact that many continuous

girders and lattice bridges were built in Europe is no proof of their excellency. In this regard we quote a part of a review of Mr. Merriman's article on continuous bridges, as contained in the *Engineering and Mining Journal* (October, 28th, 1876):

"The art of bridge-building has reached a much higher degree of perfection in this country than in Europe, and the reason for this is not difficult to find. In Europe, and particularly in Continental Europe, the design of bridge to be built is almost always under the control of the engineer in charge of the work. He makes his plans and sends them to the ironworks and machine shops, to have them executed. There is, practically, no competition, and it rarely happens that an opportunity occurs for a comparison of the relative economy of different systems. In this country, on the other hand, the method adopted is the very best possible for securing a direct and fair comparison of the relative merits of different systems. Usually, the total length of the bridge to be built, the maximum moving load it is required to carry, and the limit of strain to be allowed in the various materials to be used, are almost the only conditions given, and tenders are invited for a structure which will carry the load safely, without over-straining the material used. The utmost liberty, as to general design and details of construction, is allowed, and consequently, that system which attains, in actual practice, the greatest economy in material and construction, is generally the successful competitor. It does not, by any means, necessarily follow, that the system which secures, theoretically, the greatest economy in the use of the material, produces the cheapest structure, for account has to be taken of the facility and cheapness with which the several parts of the bridge can be made and put together, the durability of the several parts, after erection, and other conditions which at once suggest themselves to the practical engineer, though they are not always so apparent to the college professor, whose knowledge of the subject is mostly theoretical.

"Now, the idea of continuous bridges is not at all new, and there are many of our bridge engineers who, though fully acquainted with the theoretical advantages of this system under certain conditions, have yet preferred to build independent span bridges, where the competition is so close that the very smallest percentage of economy is diligently sought after. This adverse argument, though generally satisfactory to busy engineers, is not quite so to the scientific, theoretical engineer, hence the subject comes up periodically for a fresh settlement."

In deficiency of sufficiently strong arguments in favor of continuous bridges, an appeal is made to the vague popular opinion that a continuous beam supported by a number of columns must be stronger than if it were severed over the columns. In the first place this is not always true, for instance a continuous beam of two bays is just as strong for either construction, the maximum moment in both cases being $\frac{1}{8} p l^2$.

But especially in our discussion this comparison is entirely out of place, because the question is not that of continuous beams of constant section but of continuous *skeleton structures* of great depth.

This important difference has been set forth in its full strength on page 293, and no longer can be disputed. The common theory of flexure and the common theory of continuity are applicable to solid homogeneous beams, and nearly so to riveted plate girders. But these theories are not applicable to deep skeleton structures.

Hence the experiments made by the ablest engineers of civilized nations, on the deflections of solid homogeneous beams are most valuable, and allow to draw conclusions on the values of moduli and as regards other questions, whereas experiments on the deflections of skeleton trusses only show the difference between practical and imperfect theory.

The last appeal of our critic is to *theoretical writers*, on continuity.

In answer, we remark that we need no authorities on matters of exact science, except those that give us experimental results.

Physical, practical and mathematical arguments and demonstrations must be given, which the personal opinions of

theorists without practical experience cannot replace.

In as much as the authorities quoted, except *one*, are not originators of novel theories or constructions, but more properly compilators from German and other books, we wish only to make a remark about the opinion of the one who has an acknowledged standing. This is Professor Culmann, of Zurich, who, as the originator of the new branch of finding the strains in bridges, &c., by graphical methods, justly occupies a high position in his line.

In his book on graphical statics, Professor Culmann gives a chapter on the theoretical value of constructions, where also a span of a continuous bridge is treated. But the Professor expressly remarks that this chapter is only intended to show *how* theoretical comparisons of several systems could be made. He compares several kinds of trusses for the same (!) depth, he does not introduce corrections due to the web system of continuous trusses, he does not proportion those parts which have to stand tension as well as pressure for the sum of both, but only for the maximum thereof, and finally he only takes one middle span, that is the most favorable one, of a continuous bridge instead of calculating the average value of all spans. Hence conclusions from this comparison cannot be conclusive in this discussion.

If Mr. Merriman claims to have simplified the theory of continuous girders, we cannot agree with him as to his equations II and III.

The solution of equations of the first degree is but an elementary algebraic labor, such as is expected from beginners of fourteen years of age.

As regards the triangle of the coefficients of the movements of the middle piers, it is true that we have not seen it elsewhere, but we have not paid attento it, in as much as we look at it only as a curiosity without practical value, and of little, if any, scientific interest. In the question before us, which is the examination into the real value of continuous bridges, it appears only as ballast. Namely the common theory of continuity, is not correct for skeleton trusses. Again, in each individual case the reactions must be found not only under the supposition of uniform, but also of unsymmetrical loading, which latter unavoidable calculation embraces the other, relating to only one case out of many others.

What is considered more important to us is to see whether Mr. Merriman clearly understands the fundamental principles of the theory. We are sorry to say that we do think he has not studied the subject as thoroughly as we deem necessary to give strength to his assertions. The paper on the value of continuous bridges, as written for the American Society of Civil Engineers, was the result of special studies extending over a period of ten years. Its conclusions were not set forth rashly, since the writer, himself once an admirer of the theory of continuity, had to abandon it step by step.

If the critic of this paper believed to have found any error, he was entitled and he was most welcome to a discussion in a fair, strictly scientific manner, of which an inevitable condition would have been cautious adherence to facts as regards the opinions laid down in that paper.

We think Mr. Merriman would better take back his charges of slander, boomerangs and so forth, and would remember the proverb, that one who sits within a glass house must never throw stones. For what we said in reply to his unjust attack on us, was simply the plain truth.

The paper, entitled "Levees as a System for Reclaiming Lowlands," beginning on page 415 of our November issue, does not present the entire subject as designed by Mr. Bayley.

The paper is from the Journal of the Society of Civil Engineers; and the remainder will appear in due time.

REPORTS OF ENGINEERING SOCIETIES.

THE AMERICAN SOCIETY OF CIVIL ENGINEERS, at their twenty-fourth annual meeting, held in November, elected the following officers for the ensuing year: President, George S. Greene; Vice Presidents, Alexander L. Holley and Theodore G. Ellis; Secretary, G. Leverich; Treasurer, John Bogart; Directors, J. J. R. Croes, W. Milnor Roberts, William H. Paine, James O. Morse, and M. N. Forney.

Papers lately presented to the Society have been published as follows: June Journal, "Report of Committee on Tests of Iron and Steel," W. Sooy Smith, Chairman; "Reports of Committee on Uniform Method of Gauging Streams," Theodore G. Ellis, Chairman;

"Railroad Accounts and Returns," William P. Shinn; Discussion by Albert Fink, and A. D. Briggs; "Notes and Suggestions on the Croton Water Works, and Supply for the Future," Discussion by Joseph P. Davis; "Erection of the Verrugas Bridge," Discussion by Virgil G. Bogue and Lefferts L. Buck; "Qualities of Iron and Steel," William Metcalf; "Metric System of Weights and Measures," Discussion by Theodore G. Ellis; "Final Report of Committee on Form, Weight, Manufacture and Life of Rails," Ashbel Welch, Chairman; "Report of Committee on Nomenclature and Classification of Masonry," J. James R. Croes, Chairman; "Report of Committee on Failure of the Dam at Worcester, Mass.," Theodore G. Ellis, Chairman; "Report of Committee on Resistances of Railway Trains," William P. Shinn, Chairman; "A Cheap Transfer Table," William P. Shinn; "Gauging of Streams—A Special Case," David M. Greene; "Notes on the Improvement of the Mouth of the Mississippi," Discussion by Charles W. Howell; "Report of Committee on Securing National Recognition of the Society," William Milnor Roberts and Theodore G. Ellis; "The New Portage Bridge," Discussion by Charles Macdonald; "Levees as a System of Reclaiming Lowlands," Discussion by G. K. Warren and Charles G. Forshey; "Cut-offs on the Mississippi River, their Effects on the Channel Above and Below," Charles G. Forshey; "Laying Masonry in Cold Weather" and "Compressed Air Locomotives," J. Dutton Steele.

July Journal: Proceedings, "Principles of Tidal Harbor Improvement, as applied at Wilmington, Cal.," Clinton B. Sears.

August Journal: "Gauging of Streams," by D. M. Greene; "Croton Waterworks, and Supply for the Future," by J. H. Shedd, J. B. Francis, W. J. McAlpine, and others; "On the Improvement of the Mouth of the Mississippi," by E. L. Corthell; "On Hydraulic Experiments with large Apertures of Discharge," by T. G. Ellis.

September Journal: "On Levees," Discussion by C. G. Forshey, G. K. Warren, and G. W. R. Bayley; "Cut-offs on the Mississippi, their Effects on the Channel Above and Below," by C. G. Forshey.

October Journal: "Qualities of Iron and Steel," by W. Metcalf; "On the Form, Weight, Manufacture and Life of Rails," a Report by A. Welch; Discussions on "Railroad Accounts and Returns," by several members.

IRON AND STEEL NOTES.

THE NEW NOMENCLATURE OF IRON AND STEEL.—REPORT OF THE INTERNATIONAL COMMITTE APPOINTED BY THE AMERICAN INSTITUTE OF MINING ENGINEERS ON THE NOMENCLATURE OF IRON AND STEEL.

Whereas: The recent production of soft, cast, malleable compounds of iron by the Bessemer, the Siemens-Martin, and the crucible steel processes appears to demand a new nomenclature of iron compounds, for the following reasons:

1st. The term "steel," by which these soft products are commercially and professionally designated in England and in the United States, does not completely distinguish them from previously existing "steel" which would harden and temper.

2d. A nomenclature recognized in all languages seems desirable, as well for commercial as for scientific purposes, especially as law-suits, already commenced, depend on the meaning of the term "steel."

3d. Although homogeneity, due to fusion, is usually recognized and is by this committee recognized as the most definitive characteristic of both hard and soft steel, this quality may be equally well expressed in other terms, thus leaving the old term, "steel," to define the malleable compounds of iron, which will harden and temper:

Therefore, *Resolved*, That this committee recommends the following nomenclature:

I. That all malleable compounds of iron with its ordinary ingredients, which are aggregated from pasty masses, or from piles, or from any forms of iron not in a fluid state, and which will not sensibly harden and temper, and which generally resemble what is called "wrought iron," shall be called WELD-IRON (German, *Schweisseisen;* French, *fer soude*).

II. That such compounds, when they will from any cause harden and temper, and which resemble what is now called "puddled steel," shall be called WELD STEEL (German, *Schweiss-stahl;* French, *acier soude*).

III. That all compounds of iron with its ordinary ingredients, which have been cast from a fluid state into malleable masses, and which will not sensibly harden by being quenched in water, while at a red heat, shall be called INGOT IRON (German, *Flusseisen;* French, *fer fondu*).

IV. That all such compounds, when they will from any cause so harden, shall be called INGOT-STEEL (German, *Fluss-stahl;* French, *acier fondu*).

(Signed),

I. LOWTHIAN BELL, DR. HERMANN WEDDING,
P. TUNNER, RICHARD AKERMAN,
A. L. HOLLEY, THOMAS EGLESTON,
L. GRUNER.

PROTECTING IRON FROM RUST.—A method of protecting iron from rust, which is said to yield satisfactory results has been recently patented by Prof. F. S. Barff. The invention consists in the production of a protective coating or film of oxide of iron upon the surfaces of objects composed either of iron or steel, by submitting such objects when at an elevated temperature to the action of steam or by submitting the same to the action of superheated steam. Secondly, in effecting the removal or deoxidation of the oxide or oxides of iron or rust existing upon the surface of the iron or steel objects to be treated, by submitting the same at an elevated temperature to the action of hydrogen.

In carrying out the first part of the invention the objects composed of iron or steel are placed in a muffle or chamber so constructed that it may be in part or wholly closed, and so that the contents of the interior of such muffle or chamber may be raised by means of external

heat to an elevated temperature. When the objects or articles have acquired a temperature sufficiently elevated to cause the decomposition of steam or of aqueous vapor when brougbt into contact therewith, they are submitted to the action until the desired protective film or coating of oxide has been produced; or instead of submitting them to the action of heat in the manner above mentioned, they are placed in a chamber capable of being wholly or partially closed, and superheated steam is admitted to circulate therein, this operation being continued until the desired protective film or coating of oxide has been produced.

In carrying out the second part of the invention, the objects or articles, from which it is desired to remove or reduce any coating or film of oxide of iron or rust, are subjected, prior to their being submitted to the before-mentioned oxidizing process, to a deoxidizing process, and for this purpose such objects or articles are placed in a chamber or apparatus capable of being wholly or partially closed, and so constructed that the contents may be made to attain an elevated temperature by the application of external heat. When the articles have attained the desired temperature or such a temperature, as will cause the reduction of the oxide of iron to be effected by means of hydrogen, that gas is allowed to come in contact with the articles or objects until the desired reduction of the oxide has been effected. After the objects have been submitted to the first process, it is preferred to remove them when at an elevated temperature and to immerse them into oil in the case of objects or articles composed of iron or steel; the protective coating or film will thereby be rendered more tough and permanent, and in the case of objects composed of steel the temper which they will have lost may be recovered, or any desired temper may be obtained.—*English Mechanic.*

RAILWAY NOTES.

PROPOSED LIGHT RAILWAYS FOR THE WEALD OF KENT.—A project has been warmly taken up in the neighborhood of Cranbrook for a light railway to run from that town to Paddock Wood Junction, through the villages of Brenchley, Horsmonden, Lamberhurst, and Goudhurst. It is to be a single line thirteen miles in length, and constructed at a cost of £100,000. Mr. Gathorne Hardy, M P., heads the list of subscribers to the share capital with 100 shares of £10 each Mr. Butler, the engineer, stated that the first light railway which had been constructed under the Act of 1868 had now been open for six months in the eastern counties, and paid 4 per cent. dividend.

WEIGHT OF RAILWAY ENGINES AND RAILS.—The Edinburgh *Review* in a recent article on Railway Profits and Railway Losses presents some curious facts with regard to the increasing weight of the working plant of English railways:

In cases of collision it was found the old carriages suffered most. As trains became longer, therefore, and speed more rapid, it was necessary to make carriages larger and heavier; and experience has proved that increase of weight means decrease of tare. Of the carriages now used on the Metropolitan line, the first, second and third-class carriages weigh respectively 15, 14.75, and 14 25 tons, and seat 48, 64, and 80 passengers. The weight of the locomotive engine has also steadily advanced. In the Liverpool & Manchester competition of 1829, the Rocket, which won the prize, weighed 4 tons 5 cwt., and its tender 3 tons 4 cwt. Five years later, the Atlas, a six-wheeled engine on the same railway, weighed 11.4 tons in working order. At first rails weighing 35 pounds to the yard were deemed adequate, but as these could not bear a 9-ton engine, they were replaced by new rails, weighing 66 pounds per yard. In 1830, 7-ton engines were deemed heavy; in 1849, 15 ton engines; in 1850, 25 ton engines; and in 1860, 35-ton engines. Now there are good engines on the continent that weigh, exclusive of tenders, 45 tons. Mr. Meyer's 12-wheeled engine, exhibited at Vienna in 1872, weighed 60 tons. There was a weight of 10 tons to each pair of wheels, and the tractive force was estimated at 22,000 pounds, equal to the draught of 2,300 tons, exclusive of engine, on a level; 340 tons on an incline of 1 to 40; or 155 tons on one of 1 to 17. At the speed of 880 feet per minute the effect of this engine corresponds to 587 horse power.

RAILROADING IN INDIA.—The Government Director of Railways for India, Mr. Juland Danvers, has just issued his report, which contains many items of interest. The conditions of railway construction and operation in that country are to a considerable extent unique, and the study of their management and control is a matter of much interest. Mr. Danvers says that most of the roads are in good condition and carefully managed; but they sadly lack feeders and economy in operation. The total extent of railways now in existence in that country is 6,497 miles, of which 768 miles have a double track. There are 821 miles of narrow gauge road, and 6,776 of the rordinary English gauge. The total of authorized lines, when completed, will reach 9,413 miles. The capital invested in Indian roads is £105,790,929, of which £93,720,794 is guaranteed and £12,070,135 contributed by the Government.

The road has been built chiefly by two great corporations, the East India and the Great Indian Peninsula Railway Companies. These companies were organized in 1845. The companies have been aided by Government land grants carefully guarded. The Government is not to be paid for all its advances unless the railways become self-supporting, and if any company is too weak to continue its own management, it may abandon its works to Government after any portion of its road has been operated for three months. On such surrender the company gets back all the money it has actually expended on the road. The Government has the option, after twenty-five years, of purchasing the railways at a price equal to the mean value of the shares for the three years immediately previous, or it may pay a proportionate sum till the end of the period of

concession, ninety-nine years, when the railways, with their land and works, revert to the Government.

It is curious to see how small a portion of the stockholders are natives of India. Of a total number of 62,608 persons owning the stock, only 390 are natives. The net earnings of the system for 1875 amounted to £3,647,868; gross earnings, £7,412,169; working expenses, £3,764,311. The passenger earnings were £2,459,043, and the freight earnings £4,703,509. The guaranteed interest paid to the companies amounted to £4,650,346. An economy of working expenses, giving a slightly increased amount of net earnings is being gradually effected.

The system of railways in India has been rapidly developed. In 1860 there were only 849 miles. This was increased in 1861 by 760 miles of new road. The immense and profitable coal mines of India are, no doubt, to be largely developed by the railways. That development has already commenced, and is destined to grow rapidly. The great obstacle to the rapid development of the business of the roads, however, grows out of the sluggish habits of the people and their indifference to new enterprizes. For the next few years the building of additional mileage in India will be chiefly that of lateral feeders for the trunk lines already in existence.

ENGINEERING STRUCTURES.

THE NEW SUSPENSION BRIDGE AT MINNEAPOLIS.—The old Suspension bridge, built by Thos. M. Griffith, in 1854, was the first bridge that ever spanned the Mississippi. The new one now being built under direction of the same engineer, commenced in the spring of 1875 (to be completed by spring of 1877), will take rank among the best structures of the kind in the United States.

The Span will be 675 1-10 feet; towers stone, 111 feet in height; roadway twenty feet wide, forty feet above ordinary stage of water; foot walks on each side, six feet wide, made of timber grating to admit wind and dirt to pass through. The cables supporting the main bridge will be 9½ inches in diameter, those supporting the foot walks four inches in diameter. The combined ultimate strength of the cables 10,996,000 pounds. Strength of floor stays 440,000 pounds. The anchorage is obtained by passing chain links through holes slotted through a limestone ledge some ten feet thick and securing them in a tunnel below the ledge. There is also about 200 cubic yards of masonry, to each of the four anchors. This work of masonry rests on the ledge and serves to support the chains, and to deflect them to their proper position. The cost of the bridge is about $150,000.

SERIOUS ENGINEERING MISCALCULATION—THE ST. GOTHARD TUNNEL.—No blundering that has been heard of in the railroad world since some of the earliest English contracts for construction, approaches in magnitude, says the *Pall Mall Gazette*, the mistake that has been made about the cost of the St. Gothard Tunnel and the lines connected with it. The Company which has undertaken this great work, the completion of which was promised for 1880, started with a subscribed capital of 34,000,000 francs in shares, and 68 000,000 raised by debenture, together with an international subvention of 85,000,000 francs, of which Italy was to contribute more than half, while the German and Swiss governments were equally responsible for the remainder. Thus the whole estimated cost of the enterprise, 187,000,000 francs, was provided for and the works were begun with a confident assurance of speedy progress. A few months ago, however, some doubts arose as to the efficiency of the direction and the soundness of the estimates. A new "Director" was appointed, whose inquiries resulted in the conclusion that the execution of the plan in its integrity would involve an expenditure of nearly 300,000,000 francs. The Board of Administration will meet at Lucerne to consider the state of affairs; but it is difficult to see what course can be suggested for raising the new capital required.

THE HUDSON RIVER TUNNEL.—Work on this tunnel, which was begun two years ago, and interrupted by litigation, is soon to be resumed. It is entirely a private enterprise, nothing being asked from the public but right-of-way. The Board of Directors, at the head of which is Colonel Haskins, consists of eight Western capitalists, among whom are Senator Jones, of Nevada, and J. C. Parks, of the Pacific Mail Steam Ship Co. The cash capital on hand is $10,000,000, and it is estimated that about $5,000,000 more will be required for the completion of the work. The litigation arose from opposition on the part of the Morris & Essex Railroad Co. and the Jersey Shore Improvement Co. The suits have all been decided in favor of the Tunnel Co. Active operations will now be begun on the New Jersey side from the foot of Fifteenth Street. Thence the tunnel will run in a northeasterly direction, passing under the river and the Christopher Street Ferry Slip. The entrance on the New York side will be in the vicinity of Washington Square. From the New Jersey shore there will be a gradual descent of two feet in a hundred until a point 2,700 feet from New York is reached, when the tunnel will begin to ascend at the rate of one foot in every hundred. The tunnel will be about two miles in length and have a road-bed of twenty three feet, with two tracks, and the entire distance will be lit by gas. The wall overhead will be four feet thick, constructed of brick. At no point will the tunnel be less than twenty-five feet below the surface of the river, and in many places it will be seventy feet below. It is proposed to employ 200 laborers, and to continue the work day and night. In this manner the enterprise will be completed in about two years. The tunnel will be used for passengers, but its main object is rapid transportation of freight to and from the railroad depots in Jersey City.—*Railway Gazette.*

AN INTER-CONTINENTAL TUNNEL.—A pamphlet called the "Inter-Continental Tunnel"

has been published at Madrid, advocating the connection of Africa and Europe by a tunnel under the Straits of Gibraltar. It is suggested that the tunnel should start from a point between Tarifa and Algesiras on the Spanish coast, and come out between Ceuta and Tangiers on the African side. The submarine part of the tunnel would be only nine miles long, but with an incline of 1 in 100. The tunnels of approach would be nearly six miles long on each side. The maximum depth of the sea at this point is said to be 3,000 feet; and as a "crust" of 300 feet is to be left between the bottom of the sea and the arch of the tunnel, the latter will have to be 3,300 feet below the level of the sea. The originators of the scheme are of opinion that the cost will not exceed £4,000,000 sterling, and they argue that, with this tunnel and that between Calais and Dover open, there would be an overland route from London to India "without change of carriage," if desired.

ORDNANCE AND NAVAL.

THE German iron-clad gun-boat Wespe, which was launched recently, is the first of five such boats which are being built for the German Admiralty at Bremen. The object of these vessels is to defend, in connection with torpedoes, exposed parts of the coast and mouths of rivers, and to escort small expeditions in the vicinity of German harbors. It was considered necessary for this purpose that they should draw but little water, that they should be easily maneuvered, be tolerably rapid, and carry a gun capable of penetrating a very thick plate at a great distance. The Wespe is 43.5 meters long, 10.65 broad, and its draught of water is 3.1 meters. At the bow, where it would have to encounter an enemy, it is covered with an iron plate 200 millimeters thick. The deck is also plated to a thickness of eighty millimeters, and the vessel is built in water-tight compartments, with a double bottom, so as to avoid the danger of being sunk by the enemy's shot. There are two engines, together of 700 horse power, and the boat proceeds at the rate of nine knots an hour. There is sufficient storage for coal to enable the boat to go at full steam for forty hours in shallow water, or for a much longer period in deep water, where the boat may be more heavily ladened. The gun is one of the heaviest of the German naval cannon, of 30½ centimeters caliber, and it stands behind an iron-clad shield. The Wespe is to make its first trial trip next October.

THE WHITWORTH GUN.—The 35-ton gun, manufactured by Sir Joseph Whitworth for the Brazilian Government, having been tested by the French authorities, Sir Joseph applied to the French Minister of Marine for a copy of the report, but he was informed that it was contrary to custom. Therefore Colonel Dyer, late of the Royal Artillery, who witnessed the trials at Havre, made a report, which has since been compared with the official records, and is now published. On this report Sir Joseph remarks that "the bursting of the shell in the gun, mentioned in the report, proves that fluid-compressed ductile steel guns do not fly or burst explosively, although subjected to excessive strain, and while being about double the strength of wrought-iron guns, they possess all the advantages of the wrought-iron coiled guns of giving warning when strained beyond the limits of endurance."

The gun was made shorter than the regular length, in order to suit the diameter of the turret of the Independencia, and the charge used throughout the experiments was 120 lbs. pebble powder. The following results were obtained: (1) Projectile, 750 lbs.; initial velocity, 1,365 feet per second; velocity at 76 yards, 1,359 feet; velocity at 404 yards, 1,335 feet per second. (2) Projectile, 1,250 lbs.; initial velocity, 1,060 feet per second; velocity at 76 yards, 1,059 feet; velocity at 404 yards, 1,053 feet per second. (3) Projectile, 750 lbs.; elevation, 6 deg.; mean range, 3,519 yards; elevation, 15 deg. 30 min.; mean range, 7,333 yards. (4) Projectile, 1,250 lbs.; elevation, 2 deg. 31 min.; mean range 1,165 yards. Penetration: The target was composed of two thicknesses of 8-inch armor plates, fastened by screw bolts to twelve inches of oak backing on wrought-iron frames, with a further backing of twelve inches of oak.

First round: Flat-headed steel shell, empty. Struck fifty inches from the right-hand side of the target. Penetrated both plates and punched out two discs the diameter of the shell. These were forced into the backing. One disc was so deeply embedded that it could not be got out: its outer face was about twenty-eight inches from the face of the target—that is to say, at the depth of the iron framing, which it had either penetrated or driven in front of it. Two large pieces and a smaller piece of armor-plate and numerous splinters of wood were found in the front of the target. The wood work at the back of the target was much splintered. The projectile remained whole, and showed but few abrasions. It was shortened 1.6 inch, and the diameter increased 0.7 inch. The plate was struck at an angle of incidence of 10 deg. Second round: Flat-headed steel shell; bursting charge, 20 lbs. R.L.G. powder. Penetrated through the first 8-inch plate and exploded; blew one entire plate off the target, and broke it into fragments, one of which, estimated to weigh about two tons, was thrown thirty yards away. The second plate was deeply indented, and broken in several places, but the backing did not appear to suffer so much as from the first round. Third round: The shell broke up in the gun. The shell was fired empty, and, either from the plug being weak or from its not being screwed home, the thread of the screw was stripped, the whole force of the 120 lbs. of powder acted on the cavity of the shell, ultimately breaking it to pieces, but first producing a degree of expansion which caused it to act as a wedge upon the bore of the gun, lengthening, bulging and cracking the inner tube. The tube bulged 0.8 inch, and was lengthened about 0.10 inch. The thread, stripped of the screw plug, was found entire.

BOOK NOTICES.

RESISTANCE OF BOILER FLUES TO COLLAPSE. By Prof. WILLIAM CAWTHORNE UNWIN. Proceedings of Institution of Civil Engineers.

As we propose to publish this valuable paper at no distant day, an analysis of it now is unnecessary.

TECHNOLOGICAL DICTIONARY IN THE ENGLISH, GERMAN AND FRENCH LANGUAGES. By ALEXANDER TOLHAUSEN. Leipzig: Bernard Tauchnitz. New York: Henry Holt & Co.

This work is in three volumes, each containing the same terms, but presenting the languages in different order.

The number of technological terms translated is about seventy-six thousand.

The typography is pretty good, and the work compares favorably with other technological dictionaries. All such works with which we are acquainted serve better for translations from French or German into English, than for the reverse operation. The reason lies in an imperfect knowledge of the English technical terms. Thus, the work is more serviceable to English students than to either French or Germans.

REBOISEMENT IN FRANCE, OR RECORDS OF THE REPLANTING OF THE ALPS, CEVENNES AND PYRENEES, WITH TREES AND HERBAGE. By JOHN CROUMBIE BROWN. London: Henry S. King & Co. For sale by D. Van Nostrand. Price $6.25.

Part I. Resume of Surrell's Study of Alpine Torrents.

Part II. Literature relative to Alpine Torrents, and to remedial measures proposed.

Part III. Legislative and Executive measures by the Government of France, relative to Reboisement as a remedy against torrents.

Part IV. Past, Present, and Prospective Aspects of the Work.

The literature of this subject is rather meagre. This work represents the greater part of all that is available to those who read only English. The subject even now in this country demands serious attention.

RAILWAY ROLLING STOCK. From the Proceedings of the Institution of Civil Engineers. London: Printed by William Clowes & Sons.

I. On the Construction of Railway Wagons, with reference to Economy in Dead Weight. By Walter Raleigh Browne, M. A.

II. Railway Rolling Stock Capacity in relation to Dead Weight of Vehicles. By William Alexander Adams.

III. Discussion upon the Foregoing Papers.

These papers are valuable contributions to the literature of a very important subject; and notwithstanding the radical difference in method of construction between American and English cars, the lessons drawn from such experiences as are detailed in these papers, will prove exceedingly valuable on this side of the ocean.

The illustrations are excellent and very abundant.

SCIENCE LECTURES FOR THE PEOPLE. Seventh Series, 1875–6. London: Simpkin, Marshall & Co. For sale by D. Van Nostrand. Price 50 cents.

The Hulme Town Hall lectures are gladly welcomed by a wide circle of readers.

This last series is fully up to the high standing of excellence maintained in preceding years.

The lecture on the Kent Cavern is alone worth the price of the book.

The most widely useful lecture is, doubtless, the one on Food by Prof. Armstrong.

The most interesting to scientific students is probably the one on Soap Bubbles by Prof. Rucker.

THE LAW OF STORMS CONSIDERED PRACTICALLY. By W. H. ROSSER. London: Charles Wilson. For sale by D. Van Nostrand. Price $2.50.

This book is largely made up of quotations from older and well known works, together with extracts from logs of sailing vessels, some of them new but mostly of old date. There are but few generalizations of any importance, and as they are borrowed from writers of opposing views, they are in many respects contradictory.

The summary of laws and rules at the end of the volume is the most satisfactory portion of the book, considered from the practical standpoint.

The illustrations are roughly drawn, but are good enough for their purpose.

It is a book for the sailor rather than the general reader.

MATTER AND MOTION. By J. CLERK MAXWELL. London: Society for Promoting Christian Knowledge. 1876.

The author of this concise statement of the fundamental doctrines of matter and motion sets out by showing the necessity of an acquaintance with true ideas of configuration, motion and force, to the student at the commencement of his training in physical science, so that he may be able to guide the current of his thoughts along the channels of strict dynamical reasoning. The first chapter is mainly occupied with definitions; the third treats of force; and the two following on the properties of the center of mass of a material system, and of work and energy. The facts and reasonings of preceding chapters are then recapitulated in a sixth; and the two last treat successively of the pendulum and gravity, and of universal gravitation. The name of the author is a sufficient voucher for the accuracy of the work; and to this must be added that the arrangement is good, and the exposition lucid.

ASTRONOMY WITHOUT MATHEMATICS. By Sir EDMUND BECKETT, Bart. Sixth Edition. London: The Society for Promoting Christian Knowledge. 1876.

Although the principles of astronomy are taught in this treatise with as little recourse to mathematical formulæ as is practicable, that course is not followed because it is the easier one; for the author, at setting out, takes occasion to warn his reader that in this, as in other

departments of physical science, mathematics in a wide sense render the teacher's task much less difficult, constituting, as they do, in the terse phrase of the old Greek philosopher, the handles of science. What Sir Edmund aims at doing is to teach the elements of astronomy to those who are unacquainted with geometry, algebra and conic sections; and in this he has fairly succeeded. The present edition is corrected up to the existing state of the science; among other important improvements is the amendment of the dimensions, weight and distances of all the solar system beyond the moon, deduced from the observations of the transit of Venus in 1874. In connection with this the peculiar proceedings of the Astronomer Royal in regard to the choice of the method to be employed in the observations are noted and commented on. The book itself is fuller than such treatises usually are, and is addressed to a wider audience than any of the really scientific works of its class.

WOOD CONVERSION BY MACHINERY. By JOHN RICHARDS, M. E. London: J. & W. Rider. 1876.

This is a small book, formed out of a series of articles contributed to the *Timber Trades Journal*, by Mr. Richards, a well-known writer on mechanical subjects. The work is merely elementary, and yet it gives a very fair account of the different methods adopted and the machinery devised for the conversion of timber into articles of utility. As years roll on wood-converting machinery becomes more complicated, and, therefore, more delicate and expensive; yet purchasers and users of the machines rarely employ an engineer in planning their works or in adapting the machinery to the materials to be worked or to the articles to be produced, consequently there is a considerable amount of waste in production, which, considering the reduction of timber-producing land, is well worth the contemplation of the manufacturer from an economical point of view. An instance may be given in illustration: The writer, when visiting the Swedish joinery works, at one establishment came across a "child's workshop," which he thus describes: "In a long room, separated from the main building, was a row of work-benches, twenty or more in number. At each bench was a boy, from ten to sixteen years old, busy constructing wheelbarrows, boxes, toys, and other articles of wood. A master was presiding over the school, and the degree of skill manifested by children so young was marvellous. There was a forge for preparing the irons required, such as hinges, handles, tires for wheels, and so on—the factory was complete. At certain hours work stopped and school began, the two alternating as was thought expedient. The children were orphans collected throughout the city, and were here housed, clothed, fed and instructed, with the understanding that, at a certain age, and when a certain amount of efficiency had been attained, an entrance into the main works would be allowed." Thus was utilized the waste labor of the orphans and the waste wood of the works, an idea better than our uniform plan of making the gathered waifs and strays of our cities into rough choppers of firewood only, who cannot be sent to the Colonies because every man there does his own chopping for the pot and kettle, and only employs a carpenter for the higher kinds of labor.

MISCELLANEOUS.

THE engineer in charge of the Severn Tunnel works reports that the heading has now been driven to a length of 2,540 feet from the shaft, and is more than sixty feet under the shoots. The strata for the last 500 feet has been almost free from water. The Pennant Rock continues to be of the same strong character as before, and has become even closer and harder as the heading has neared the shoots. This condition of the rock is most favorable to the success of the work. Inclusive of the cost of the plant, the expenditure to the present time amounts to £55,448.

JAPANESE HEMP.—Japan produces hemp of the finest quality, and probably, when machinery has been brought to bear on the industry, it will compete favorably with Manilla hemp. The plant is perennial, and attains a height of six feet and upwards; the stem is covered with a short hairy substance; the leaves are heart-shaped, with a sharp point, their surface being of a bluish color, and the back white; both sides are furry and rough to the touch. In the summer small sprouts of about two or three inches in height appear at the point where the leaves join the stem, and throw out blossoms, which develop into small white flowers. Mr. Consul Robertson gives the following account of the manner in which the fibre is obtained: When the summer has set in, the plantation is fired, after which the ground is well prepared with manure, and left till the close of the summer, when the shoots will have attained their full height. They are then cut and soaked in running water for about four hours. After immersion the stalks are broken in about three places, by which means the rind is separated from the pith. In the interstice thus made the thumb of the left hand is inserted, and the stalks shredded. The shredded parts are placed in layers, and are next laid on a board which has a foot piece at one end, so as to make an inclined plane. A small edged tool is then grasped in the right hand, the shreds being firmly held down with the left, and the inner white coating is scraped off. The shreds are now hung upon a frame, after which they are again placed on the board, and this time the outer green pitch is scraped off. The fibre is then tied together in bundles and dried. This dried fibre is woven into cloth and all kinds of piece goods. The outer green bark or peel is also dried, macerated, and made into paper pulp, being used for the manufacture of the coarsest kinds of papers. It is sometimes used in its dried state by the poorer classes as a stuffing for mattresses. The best of the outer or surface fibre is also made up into a material very strong in texture and of a mouse color. The pith, or what is left after obtaining the fibre, is utilized in finishing off the thatch of houses.—*All the Year Round.*